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An interesting question, which has waited thousands of years even to be asked, and may wait still longer for an all-sufficient answer, relates to the motion of what were once called the fixed stars. If numbers count for anything, this is the grandest problem which can be presented to the mind of the astronomer. The argument from probabilities, which reposes on a substantial mathematical foundation, is loud in affirming some kind of motion, and repudiates the notion of absolute rest. We must place the stars outside the pale of science, and where no process of reasoning can reach them, or we must suppose that they subscribe to the universal law of all matter which we know, and exert attractive or repulsive forces upon each other. There may be one solitary body, or more probably an ideal point of space, the centre of gravity of the material universe, around which there is equilibrium: but everywhere else there must be motion. Though distance may reduce the effect of each one of the forces to a minimum, in the aggregate their influence will not be insignificant. The sun must share the common lot of the stars unless we repeat the folly of ancestral science, at which we now smile, and transfer

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the throne of the heaven of matter from the earth to the centre of our own little system. If the sun move, a new order of parallactic motion springs up in sidereal astronomy. The process of elimination requires the mathematician to calculate the direction and velocity of the motion of the sun which will leave behind it the smallest unexplained residuum: and this remainder is the motion of the stars themselves. The delicacy of the problem lies in the minuteness of the quantities to be observed and in the assumptions which must be made in regard to the distances of the stars; only a few of which have been positively computed from parallax. However, a result has been reached, highly probable in the sun's case, but which can be converted into absolute values for other stars only so fast as their individual distances are discovered. Here again physics and chemistry, with the spectroscope in hand, have come to the aid of astronomy and geometry. Should it appear that the conclusions from spectrum analysis must be questioned, the attempt was brave, and even a defeat would be honorable.

In 1675, a Danish astronomer observed the novel fact that the frequency in the eclipses of Jupiter's satellites fluctuated with the motion of the planet to or from the earth. He hit upon a happy explanation, viz: that the swift light takes more or less time to telegraph the astronomical news across the omnipresent lines of force. This early observation is the avant-courier of a host of others which have slowly followed in close array. That of a blind musician comes next. He noticed, in 1835, that the pitch of a steam-whistle, on the Lowell Railroad, fell suddenly as the locomotive passed him. Unfortunately, Munroe's observation was never published, although he sought and found an explanation of what was then a strange fact. In this case, the whistle sends the message, the waves of sound transmit it, and the ear is the register: but the changing distance modifies the time. In 1842, Doppler of Prague was led, by theoretical considerations, to formulate the proposition, now known in science as Doppler's principle: that the color of light and the pitch of sound, as they tell upon the senses, are changed by the relative velocity of the observer and the origin of the disturbance. In 1845, Buy Ballot made experiments upon the railroads in the Netherlands, and Scott Russell repeated them on English railroads, which confirmed the theory in the case of sound. In the application of the

theory to color, few astronomers will be willing to follow Doppler in all his extravagancies.

If it be true, theoretically, that the relative velocity of light, the wave-length of transmission, and the period of oscillation in the ether, are altered by the relative motion of the observer and the place from which the undulation starts, it is obvious that all other velocities have but a small chance in competition with the velocity of light, and that slight changes of color, if physically real as Doppler supposed, would fail of being recognized even by the eye of a painter. To interpose the spectroscope, and observe the change of refrangibility by the displacement of the sharp lines of the spectrum, was a lucky escape from this embarrassment. After Huggins had tried his hand at this new method, with a small telescope, upon the brightest of all the stars, he was supplied by the Royal Society of London with a larger instrument to pursue the investigation. The results of his spectroscopic inquiry into the motions of many stars have been published. Where these results have conflicted with the foregone conclusions of astronomy, Huggins has not hesitated to arraign the accuracy of astronomical data and methods. I have freely admitted the delicacy and difficulty of the geometrical process. The spectroscopic analysis, when applied to the same problem, walks upon slippery ground and must take heed lest it also fall. The alleged displacement is a nice quantity, and instrumental sources of error have been pointed out which may explain away the whole of it. I lay no stress upon the large difference between Vogel and Huggins in the *quantity* of motion which spectrum analysis ascribes to Sirius, inasmuch as the direction of the motion is the same. We do not yet know all the elements which the earth contains. The spectroscope has already added four to the number. There is reason to think that the stars, though having some substances in common with the earth and sun, are not without their peculiarities. The lines in the stellar spectra may be out of position, not because they are the displaced lines of sodium, magnesium, and hydrogen, but in consequence of novelties in the gaseous atmospheres of the stars. Still, there will be a presumption, perhaps a probability, in favor of Huggins' deduction, if it rest on a sound basis of theory. If there is any weakness in the physical and mathematical foundation of his argument,

gratifying as it is to the imagination and the aspirations of science, the whole superstructure must fall.

I am thus suddenly brought face to face, with the second head of my subject: the mathematical and philosophical state of the physical sciences.

The luminiferous ether and the undulatory theory of light have always troubled what is supposed to be the imperturbable character of the mathematics. The proof of a theory is indisputable when it can predict consequences, and call successfully upon the observer to fulfil its prophecies. It is the boast of astronomers that the law of gravitation thus vindicates itself. The undulatory theory of light has shown a wonderful facility of adaptation to each new exigency in optics, and has opened the eye of observation to see what might never have been discovered without the promptings of theory. But this doctrine, and that of gravitation also, have more than once been arrested in their swift march and obliged to show their credentials. After Fresnel and Young had secured a firm foothold for Huyghens' theory of light in mechanics and experiment, questions arose which have perplexed, if not baffled, the best mathematical skill. How is the ether affected by the gross matter which it invests and permeates? Does it move when they move? If not, does the relative motion between the ether and other matter change the length of the undulation or the time of oscillation? These queries cannot be satisfactorily answered by analogy, for analogy is in some respects wanting between the ether and any other substance. Astronomy says that aberration cannot be explained unless the ether is at rest. Optics replies that refraction cannot be explained unless the ether moves. Fresnel produced a reconciliation by a compromise. The ether moves with a *fractional* velocity large enough to satisfy refraction, but too small to disturb sensibly the astronomer's aberration. In 1814, Arago reported to Fresnel that he found no sensible difference in the prismatic refraction of light, whether the earth was moving with full speed towards a star or in the opposite direction, and asked for an explanation. Fresnel submitted the question to mathematical analysis, and demonstrated, that whatever change was produced by the motion of the prism in the relative velocity of light, the wave-length in the prism, and the refraction, was compensated by the physiological aberration when the rays

emerged. Very recently, Ketteler of Bonn has gone over the whole ground again with great care, studying not only Arago's case but the general one, in which the direction of the light made any angle with the motion of the earth: and he proves that the light will always enter the eye in the same apparent direction as it would have done if the earth were at rest. The mathematical and physical view taken of this subject by Fresnel, has been under discussion for sixty years, and forty eminent physicists and mathematicians might be enumerated who have taken part in it. Fresnel's explanation has encountered difficulties and objections. Still, it is consistent not only with Arago's negative result but with the experiments on diffraction by Fizeau and Babinet, and the preponderance of mathematical evidence is on that side. Mr. Huggins runs counter to the general drift of physical and algebraical testimony (although he appears to be sustained by the high authority of Maxwell), when he attributes some displacement of the spectrum lines to the motion of the earth, and qualifies the observed displacement on that account. The number of stars which Huggins has observed is insufficient for any sweeping generalization. And yet he seems inclined to explain the revelations of his spectroscope, not by the motion of the stars, but by that of the solar system: because those stars which are in the neighborhood of the place in which astronomers have put the solar apex are moving, apparently, towards the earth, while those in the opposite part of the sky recede. If it be true that the earth's annual motion produces no displacement in the spectrum, then the motion of the solar system produces none. Or, waiving this objection, if the correct explanation has been given by Huggins, astronomers have failed, by their geometrical method, of rising to the full magnitude of the sun's motion. The discrepancy appears to awaken no distrust in Mr. Huggins' mind as to the delicacy of the spectrum analysis or the mathematical basis of his reasoning. On the contrary, he would remove the discrepancy by throwing discredit on the estimate of star-distances made independently by Struve and Argelander from different lines of thought.

Next we ask, if it is certain that even the motion of the luminary will change the true wave-length, the period of oscillation, and the refrangibility, of the light which issues from it. The commonly received opinion on this subject has not been allowed to pass unchallenged. It is fortified by more than one analogy:

but it is said that comparison is not always a reason. It is not denied that, when the sonorous body is approaching, the sound waves are shortened, the number of impulses on the ear by the condensed air is increased, and the pitch of the sound is raised. Possibly, the color of light would follow the same law; but there is no experiment to prove it, and very little analogy exists between the eye and the ear. There is no analogy, whatever, between the subjective sensation by either organ and the physical action of the prism. The questions at issue are these:—Does refraction depend upon the absolute or the relative velocity of light; are the time of oscillation of the particles of ether and the normal wave-length, corresponding to it, changed by any motion of translation in the origin; or is the conservation of these elements an essential attribute of the luminiferous medium. It has been said that Doppler reasoned as if the corpuscular theory of light were true, and then expressed himself in the language of undulations. Evidently, there is an obscurity in the minds of many physicists, and an uncertainty in all, when they reason upon the mechanical constitution of the ether, and the fundamental laws of light. The mathematical theory is not so clear as to be able to dispense with the illumination of experiment. Within the present year, Van der Willigen has published a long and well considered memoir on the theoretical fallacies which vitiate the whole of Huggins' argument for the motion of the stars and nebulæ. His analysis proves that the motion of the luminary will not interfere with the time of oscillation and the wave-length, provided that the origin of the disturbance is not a mathematical point but a vibrating molecule, and that the sphere of action of this molecule upon surrounding molecules is large enough to keep them under its influence during ten or a hundred vibrations, before it is withdrawn by the motion of translation. If this theoretical exposition of the subject should be generally adopted by mathematicians, the spectroscopic observations on the supposed motion of the stars must receive another interpretation. On the other hand, if a luminary is selected which is known to move, independently of spectroscopic observations, and the displacement of the spectrum lines accords with this motion, it will be time to reconsider the mathematical theory, and make our conceptions of the ether conform to the experiment. The spectroscopic observation of Angström on an oblique electric spark does not favor Huggins' views. Secchi testifies to opposite

displacements when he examined, with a direct vision spectroscope, the two edges of the sun's equator, one of which was rotating towards him and the other from him, and Vogel has repeated the observation with a reversion-spectroscope. This would have the force of a crucial experiment were it not that an equal displacement was seen on other parallels of latitude, and that the bright bands of the chromosphere were moved but not the dark lines of the solar atmosphere.

When Voltaire visited England in 1727 he saw at the universities the effect of Newton's revolutionary ideas in astronomy. The mechanism of gravitation had exiled the fanciful vortices of Descartes, which were still circulating on the continent. So he wrote: "A Frenchman who comes to London finds many changes in philosophy as in other things: he left the world full, he finds it empty." The same comparison might be made now, not so much between nationalities as between successive stages of scientific development. At the beginning of this century the universe was as empty as an exhausted receiver: now it has filled up again. Nature's abhorrence of a vacuum has been resuscitated, though for other reasons than those which satisfied the Aristotelians. It is the mathematicians and not the metaphysicians who are now discussing the relative merits of the *plenum* and the *vacuum*. Newton in his third letter to Bentley wrote in this wise:—"That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance, through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man, who has in philosophical matters a competent faculty of thinking, can ever fall into it." Roger Cotes, who was Newton's successor in the chair of mathematics and natural philosophy at Cambridge, was only four years old when the first edition of the *Principia* was issued, and Newton outlived him by ten years. The venerable teacher pronounced upon the young mathematician, his pupil, these few but comprehensive words of eulogy: "If Cotes had lived, we should have known something." The view taken of gravitation by Cotes was not the same as that held by his master. He advocated the proposition that action at a distance must be accepted as one of the primary qualities of matter, admitting of no farther analysis. It was objected by Hobbes and other metaphysicians,

that it was inconceivable that a body should act where it was not. All our knowledge of mechanical forces is derived from the conscious effort we ourselves make in producing motion. As this motion employs the machinery of contact, the force of gravitation is wholly outside of all our experience. The advocates of action at a distance reply, that there is no real contact in any case, that the difficulty is the same with the distance of molecules as that of planets, that the mathematics are neither long-sighted nor short-sighted, and that an explanation which suits other forces is good enough for gravitation.

Comte extricated himself from this embarrassment by excluding causes altogether from his positive philosophy. He rejects the word attraction as implying a false analogy, inconsistent with Newton's law of distance. He substitutes the word gravitation, but only as a blind expression by which the facts are generalized. According to Comte's philosophy, the laws of Newton are on an equality with the laws of Kepler, only they are more comprehensive, and the glory of Kepler has the same stamp as that of Newton. Hegel, the eminent German metaphysician, must have looked at the subject in the same light when he wrote these words: — "Kepler discovered the laws of free motion; a discovery of immortal glory. It has since been the fashion to say that Newton first found out the truth of these rules. It has seldom happened that the honor of the first discoverer has been more unjustly transferred to another." Schelling goes farther in the same direction: he degrades the Newtonian law of attraction into an empirical fact, and exalts the laws of Kepler into necessary results of our ideas.

Meanwhile, the Newtonian theory of attraction, under the skilful generalship of the geometers, went forth on its triumphal march through space, conquering great and small, far and near, until its empire became as universal as its name. The whirlpools of Descartes offered but a feeble resistance, and were finally dashed to pieces by the artillery of the parabolic comets; and the rubbish of this fanciful mechanism was cleaned out as completely as the cumbrous epicycles of Ptolemy had been dismantled by Copernicus and Kepler. The mathematicians certified that the solar system was protected against the inroads of comets, and the border warfare of one planet upon another, and that its stability was secure in the hands of gravitation, if only space should be

kept open, and the dust and cobwebs which Newton had swept from the skies should not reappear. Prophetic eyes contemplated the possibility of an untimely end to the revolution of planets, if their ever expanding atmospheres should rush in to fill the room vacated by the maelstroms of Descartes. When it was stated that the absence of infinite divisibility in matter, or the coldness of space, would place a limit upon expansion, and, at the worst, that the medium would be too attenuated to produce a sensible check in the headway of planets, and when, in more recent times, even Encke's comet showed but the slightest symptoms of mechanical decay, it was believed that the motion was, in a practical, if not in a mathematical sense, perpetual. Thus it was that the splendors of analysis dimmed the eyes of science to the intrinsic difficulties of Newton's theory, and familiarity with the language of attraction concealed the mystery that was lurking beneath it. A long experience in the treatment of gravitation had supplied mathematicians with a fund of methods and formulas suited to similar cases. As soon as electricity, magnetism and electro-magnetism took form, they also were fitted out with a garment of attractive and repulsive forces acting at a distance: and the theories of Cavendish, Poisson, Aepinus and Ampere, endorsed as they were by such names as Laplace, Plana, Liouville and Green, met with general acceptance.

The seeds, which were destined to take root in a later generation, and disturb if not dislodge the prevalent interpretation of the force of gravitation, were sown by a contemporary of Newton. They found no congenial soil in which they could germinate and fructify until the early part of the present century. At the present moment, we find the luminiferous ether in quiet and undivided possession of the field from which the grosser material of ancient systems had been banished. The *plenum* reigns everywhere; the vacuum is nowhere. Even the corpuscular theory of light, as it came from the hands of its founder, required the reinforcement of an ether. Electricity and magnetism, on a smaller scale, applied similar machinery. If there was a fundamental objection to the conception of forces acting at a distance, certainly the bridge was already built by which the difficulty could be surmounted. The turning-point between the old physics and the new physics was reached in 1837, when Faraday published his experiments on the specific inductive capacity of substances.

This discovery was revolutionary in its character, but it made no great stir in science at the time. The world did not awake to its full significance until the perplexing problem of ocean telegraphs converted it from a theoretical proposition into a practical reality, and forced it on the attention of electricians. The eminent scientific advisers of the cable companies were the first to do justice to Faraday. This is one of the many returns made to theoretical electricity for the support it gave to the most magnificent commercial enterprise.

The discovery of diamagnetism furnished another argument in favor of the new interpretation of physical action. What that new interpretation was is well described by Maxwell. "Faraday, in his mind's eye, saw lines of force traversing all space, where the mathematicians saw centres of force attracting at a distance; Faraday saw a medium where they saw nothing but distance; Faraday sought the seat of the phenomena in real actions going on in the medium, they were satisfied that they had found it in a *power* of action at a distance impressed on the electric fluids." The physical statement waited only for the coming of the mathematicians who could translate it into the language of analysis, and prove that it had as precise a numerical consistency as the old view with all the facts of observation. A paper published by Sir William Thomson, when he was an undergraduate at the university of Cambridge, pointed the way. Prof. Maxwell, in his masterly work on electricity and magnetism, which appeared in 1873, has built a monument to Faraday, and unconsciously to himself also, out of the strongest mathematics. For forty years mathematicians and physicists had labored to associate the laws of electrostatics and electrodynamics under some more general expression. An early attempt was made by Gauss in 1835, but his process was published, for the first time, in the recent complete edition of his works. Maxwell objects to the formula of Gauss because it violates the law of the conservation of energy. Weber's method was made known in 1846; but it has not escaped the criticism of Helmholtz. It represents faithfully the laws of Ampere and the facts of induction, and led Weber to an absolute measurement of the electrostatic and electromagnetic units. The ratio of these units, according to the formulas, is a velocity; and experiment shows that this velocity is equal to the velocity of light. As Weber's theory starts with the conception of action at a distance,

without any mediation, the effect would be instantaneous, and we are at a loss to discover the physical meaning which he attaches to his velocity. Gauss abandoned his researches in electromagnetism because he could not satisfy his mind in regard to the propagation of its influence in time. Other mathematicians have worked for a solution, but have lost themselves in a cloud of mathematical abstraction. The two theories of light have exhausted all imaginable ways in which force can be gradually transmitted without increase or loss of energy. Maxwell cut the Gordian knot when he selected the luminiferous ether itself as the arena on which to marshal the electromagnetic forces under the symbols of his mathematics, and made light a variety of electromagnetic action. His analysis gave a velocity essentially the same as that of Weber, with the advantage of being a physical reality and not a mere ratio. Of the two volumes of Mr. Maxwell, freighted with the richest and heaviest cargo, the reviewer says: "Their author has, as it were, flown at everything: and, with immense spread of wing and power of beak, he has hunted down his victims in all quarters, and from each has extracted something new and interesting for the intellectual nourishment of his readers." Clear physical views must precede the application of mathematics to any subject. Maxwell and Thomson are liberal in their acknowledgments to Faraday. Mr. Thomson says: "Faraday, without mathematics, divined the result of the mathematical investigation; and, what has proved of infinite value to the mathematicians themselves, he has given them an articulate language in which to express their results. Indeed, the whole language of the *magnetic field* and *lines of force* is Faraday's. It must be said for the mathematicians that they greedily accepted it, and have ever since been most zealous in using it to the best advantage."

It is not expected that the new views of physics will be generally accepted without vigorous opposition. A large amount of intellectual capital has been honestly invested in the fortunes of the other side. The change is recommended by powerful physical arguments, and it disenthralls the theories of science from many metaphysical difficulties which weigh heavily on some minds. On the other hand, the style of mathematics which the innovation introduces is novel and complex; and good mathematicians may find it necessary to go to school again before they can read and understand the strange analysis. It is feared that with many who

are not easily deflected from the old ruts, the intricacies of the new mathematics will outweigh the superiority of the new physics.

The old question, in regard to the nature of gravitation, was never settled: it was simply dropped. Now it is revived with as much earnestness as ever, and with more intelligence. Astronomy cast in its own mould the original theories of electrical and magnetic action. The revolution in electricity and magnetism must necessarily react upon astronomy. It was proved by Laplace, from data which would now, probably, require a numerical correction, that the velocity of the force of gravitation could not be less than eight million times the velocity of light; in fact, that it was infinite. Those who believe in action at a distance cannot properly speak of the transmission of gravitation. Force can be transmitted only by matter: either with it or through it. According to their view, action at a distance *is* the force, and it admits of no other illustration, explanation, or analysis. It is not surprising that Faraday and others, who had lost their faith in action at short distances, should have been completely staggered by the ordinary interpretation of the law of gravitation, and that they declared the clause which asserted that the force diminished with the square of the distance to be a violation of the principle of the conservation of force.

Must we then content ourselves with the naked facts of gravitation, as Comte did, or is it possible to resolve them into a mode of action in harmony with our general experience, and which does not shock our conceptions of matter and force? In 1798, Count Rumford wrote thus: "Nobody surely, in his sober senses, has ever pretended to understand the mechanism of gravitation." Probably Rumford had never seen the paper of Le Sage, published by the Berlin Academy in 1782, in which he expounded his mechanical theory of gravitation, to which he had devoted sixty-three years of his life. In a posthumous work, printed in 1818, Le Sage has developed his views more fully. He supposed that bodies were pressed towards one another by the everlasting pelting of ultramundane atoms, inward bound from the immensity of space beyond, the faces of the bodies which looked towards each other being mutually screened from this bombardment. It was objected to this hypothesis, which introduced Lucretius into the society of Newton and his followers, that the collision of atoms with atoms, and with planets, would cause a secular diminution in the force of

gravity. Le Sage admitted the fact. But as no one knew that the solar system was eternal, the objection was not fatal. As the necessity for giving a mechanical account of gravitation was not generally felt at the time, the theory of Le Sage fell into oblivion. In 1873, Sir William Thomson resuscitated and republished it. He has fitted it out in a fashionable dress, made out of elastic molecules instead of hard atoms, and has satisfied himself that it is consistent with modern thermo-dynamics and a perennial gravitation.

Let us now look in a wholly different quarter for the mechanical origin of gravitation. In 1870, Prof. Guthrie gave an account of a novel experiment, viz:—the attraction of a light body by a tuning-fork when it was set in vibration. Thomson repeated the experiment upon a suspended eggshell and attracted it by a simple wave of the hand. Thomson remarks "that what gave the great charm to these investigations, for Mr. Guthrie himself, and no doubt also for many of those who heard his expositions and saw his experiments, was, that the results belong to a class of phenomena to which we may hopefully look for discovering the mechanism of magnetic force, and possibly also the mechanism by which the forces of electricity and gravity are transmitted." By a delicate mathematical analysis, Thomson arrives at the theorem that the "average pressure at any point of an incompressible, frictionless fluid, originally at rest, but set in motion and kept in motion by solids, moving to and fro, or whirling round in any manner, through a finite space of it," would explain the attractions just described. Moreover, he is persuaded by other effects besides those of light, that, in the interplanetary spaces and in the best artificial vacuum, the medium which remains has "perfectly decided mechanical qualities, and, among others, that of being able to transmit mechanical energy, in enormous quantities:" and he cherishes the hope that his mathematical theorems on abstract hydrokinetics are of some interest in physics as illustrating the great question of the eighteenth and nineteenth centuries:—Is action at a distance a reality, or is gravitation to be explained, as we now believe magnetic and electric forces must be, by action of intervening matter?

In 1869 and 1873, Prof. Challis of Cambridge, England, published two works on the Principles of Mathematical Physics. They embody the mature reflexions of a mathematical physicist

at the advanced age of threescore years and ten. Challis believes that there is sufficient evidence for the existence of ether and atoms as physical realities. He then proceeds to say :—"The fundamental and only admissible idea of *force* is that of *pressure*, exerted either actively by the ether against the surface of the atoms, or as reaction of the atoms on the ether by resistance to that pressure. The principle of deriving fundamental physical conceptions from the indications of the senses does not admit of regarding *gravity*, or any other force varying with distance, as an essential quality of matter, because, according to that principle, we must, in seeking for the simplest idea of physical force, have regard to the sense of *touch*. Now, by this sense, we obtain a perception of force as pressure, distinct and unique, and not involving the variable element of distance, which enters into the perception of force as derived from the sense of sight alone. Thus, on the ground of simplicity as well as of distinct perceptibility, the fundamental idea of force is pressure." As all other matter is passive except when acted upon by the ether, the ether itself, in its quiescent state, must have uniform density. It must be coextensive with the vast regions in which material force is displayed. Challis had prepared himself for the elucidation and defence of his dynamical theory by a profound study of the laws of motion in elastic fluids. From the mathematical forms in which he has expressed these laws he has attempted to derive the principal experimental results in light, heat, gravitation, electricity, and magnetism. Some may think that Mr. Challis has done nothing but clothe his theory in the cast off garments of an obsolete philosophy. If its dress is old, it walks upon new legs. The interplay between ether and atoms is now brought on to the stage, not as a speculation supported by metaphysical and theological arguments, but as a physical reality with mathematical supports. I should do great injustice to this author if I left the impression that he himself claimed to have covered the whole ground of his system by proof. Mathematical difficulties prevented him from reaching a numerical value for the resultant action of a wave of ether upon the atom. What he has written is the guidepost, pointing the direction in which science is next to travel: but the end of the journey is yet a great way off. The repeated protests of Mr. Challis against the popular physics of the day, and his bold proclamation of the native, independent motion of the ether,

have aroused criticism. What prevents the free ether, asks the late Sir John Herschel, from expanding into infinite space? Mr. Challis replies that we know nothing about infinite space or what happens there, but the existence of the ether, where our experience can follow it, is a physical reality. The source of the motion which the ether acquires is not the sun: for the most efficient cause of solar radiation is gravitation and condensation. Our author avoids the vicious circle of making gravitation, first the reason and afterwards the consequence of the motion of the ether. He says: "It follows that the sun's heat, and the heat of masses in general, are stable quantities, oscillating, it may be, like the planetary motions, about *mean* values, but never permanently changing, so long as the Upholder of the universe conserves the force of the ether and the qualities of the atoms. There is no law of destructibility: but the same Will that conserves can in a moment destroy." The following remarks upon this theory deserve our attention. "The explanation of any action between distant bodies by means of a clearly conceivable process, going on in the intervening medium, is an achievement of the highest scientific value. Of all such actions that of gravitation is the most universal and the most mysterious. Whatever theory of the constitution of bodies holds out a prospect of the ultimate explanation of the process by which gravitation is effected, men of science will be found ready to devote the whole remainder of their lives to the development of that theory."

The hypotheses of Challis and Le Sage have one thing in common; the motion of the ether and the driving storm of atoms must come from outside the world of stars. "On either theory, the universe is not even temporarily automatic, but must be fed from moment to moment by an agency external to itself." Our science is not a finality. The material order which we are said to know makes heavy drafts upon an older or remoter one, and that again upon a third. The world, as science looks at it, is not self-sustaining. We may abandon the hope of explaining gravitation, and make attraction itself the primordial cause. Our refuge then is in the sun. When we qualify the conservation of energy by the dissipation of energy, the last of which is as much an induction of science as the first, the material fabric which we have constructed still demands outward support. Thomson calculates that, within the historical period, the sun has emitted hundreds of

times as much mechanical energy as is contained in the united motions of all the planets. This energy, he says, is dissipated more and more widely through endless space, and never has been, probably never can be, restored to the sun, without acts as much beyond the scope of human intelligence as a creation or annihilation of energy, or of matter itself, would be.

From the earliest dawn of intellectual life, a general theory of the constitution of matter has been a fruitful subject of debate, and human science and philosophy have ever been dashing their heads against the intractable atoms. The eagerness of the discussion was the greater, the more hopeless the solution. For every man who set up an hypothesis upon the subject there were half a dozen others to knock it down; until at last speculation, which bore no fruit, was suspended. A lingering interest still hung around the question, whether matter was not infinitely divisible, and the atomic philosophers were not chasing a chimera. From every new decision on this single point there was an appeal, and the foothold which the atoms had secured in chemistry was gradually subsiding. Of a sudden, the atomic theory has gained a new lease of life. But the hero of the new drama is not the atom but the molecule. In all the physical sciences, including astronomy, the war has been carried home to the molecules: and the intellectual victories of this and the next generation will be on this narrow field. From the outlying provinces of physics; from the sun, the stars and the nebulæ; from the comets and meteors; from the zodiacal light and the aurora; from the exquisitely tempered and mysterious ether; the forces of nature have been moving in converging lines to this common battle ground, and some shouts of victory have already been heard. In the long and memorable controversy between Newton and Leibnitz, and their adherents, as to the true measure of force, it was charged against the Newtonian rule that force was irrecoverably lost whenever a collision occurred between hard, inelastic bodies. The answer was, that nature had anticipated the objection and had avoided this kind of matter. Inelastic bodies were yielding bodies, and the force which had disappeared from the motion had done its work in changing the shape. But unless the body could recover its original figure by elasticity, there was no potential energy and force was annihilated. It is now believed, and to a large extent demonstrated, that the force, apparently lost, has been transformed into heat, electricity or some

other kind of molecular motion, of which the change of shape is only the outward sign. The establishment on a firm foundation of theory and experiment of the so-called conservation of energy, the child of the correlation of physical forces, is one of the first fruits of molecular mechanics.

It is no disparagement of this discovery, on which was concentrated the power of several minds, to call it an extension, though a vast one, of Newton's law of inertia, of Leibnitz's *vis viva*, and of Huyghens' and Bernouilli's conservation of living forces; these older axioms of mechanics having free range only in astronomy, where friction, resistance and collision do not interfere. The conservation of energy, in its extended signification, promises to be, like its forerunners, a valuable guide to discovery, especially in the dark places into which physical science has now penetrated. The caution which Lagrange has given in reference to similar mechanical principles, such as the conservation of the motion of the centre of gravity, the conservation of moments of rotation, the preservation of areas, and the principle of least action, is not without its applicability to the new generalization. Lagrange accepts them all as results of the known laws of mechanics and not as the essence of the laws of nature. The most that physical science can assert is that it possesses no evidence of the destructibility of matter or force.

It is not pretended that the existence of atoms has been or can be proved or disproved. Some chemists think that the atomic theory is the life of chemistry: others have abandoned it. Its importance is lost in that of the molecular theory. And what has this accomplished to justify its existence? If we define the molecule of any substance as the smallest mass of that substance which retains all its chemical properties, we can start with the extensive generalization of Avogadro and Ampere, that an equal volume of every kind of matter in the state of vapor, and under the same pressure and temperature, contains an equal number of such molecules. The conception of matter as consisting of parts, which are perpetually flying over their microscopic orbits and producing by their fortuitous concourse all the observed qualities of bodies, is as old as Lucretius. He saw the magnified symbol of his hypothesis in the motes which chase one another in the sunbeam. One of the Bernouillis thought that the pressure of gases might be caused by the incessant impact of these little masses on the vessel

which held them. The discovery that heat was a motion and not a substance, foreshadowed by Bacon, made probable by Rumford and Davy, and rigidly proved by Mayer and Joule when they obtained its exact mechanical equivalent, opened the way to the dynamical theory of gases. Joule calculated the velocity of this promiscuous artillery, rendered harmless by the minuteness of the missiles, and found that the boasted guns of modern warfare could not compete with it. Clausius consummated the kinetic theory of gases by his powerful mathematics, and derived from it the experimental laws of Mariotte, Gay-Lussac and Charles. By the assumption of data, more or less plausible, several mathematicians have succeeded in computing the sizes and the masses of the molecules and some of the elements of their motion. It should not be forgotten that mathematical analysis is only a rigid system of logic by which wrong premises conduct the more surely to an incorrect conclusion. To claim for all the conclusions which have been published in relation to the molecules the certainty which fairly belongs to some of them would prejudice the whole cause.

One of the most interesting investigations in molecular mechanics was published by Helmholtz in 1858. It is a mathematical discussion of what he calls ring-vortices in a perfect, frictionless fluid. Helmholtz has demonstrated that such vortices possess a perpetuity and an inviolability once thought to be realized only by the eternal atoms. The ring-vortices may hustle one another, and pass through endless transformations, but they cannot be broken or stopped. Thomson seized upon them as the impersonation of the indestructible but plastic molecule which he was looking for, to satisfy the present condition of physical science. The element of the new physics is not an atom or a congeries of atoms but a whirling vapor. The molecules of the same substance have one invariable and unchangeable mass: they are all tuned to one standard pitch and, when incandescent, emit the same kind of light. The music of the spheres has left the heavens and condescended to the rhythmic molecules. There is here no birth or death or variation of species. If other masses than the precise ones which represent the elements have been eliminated, where, asks Maxwell, have they gone? The spectroscope does not show them in the stars or nebulæ. The hydrogen and sodium of remotest space are in unison with the hydrogen and sodium of earth.

In the phraseology of our mechanics we define matter and force

as if they had an independent existence. But we have no conception of inert matter or of disembodied force. All we know of matter is its pressure and its motion. The old atom had only potential energy; the energy of its substitute, the molecule, is partly potential and partly kinetic. If it could be shown that all the phenomena displayed in the physical world were simply transmutations of the original energy existing in the molecules, physical science would be satisfied. Where physical science ends, natural philosophy, which is not wholly exploded from our vocabulary, begins. Natural philosophy can give no account of energy when disconnected with an ever present Intelligence and Will. In Herschel's beautiful dialogue on atoms, after one of the speakers had explained all the wonderful exhibitions of nature as the work of natural forces, Hermione replies: — "Wonderful, indeed! Anyhow, they must have not only good memories but astonishing presence of mind, to be always ready to act, and always to act, without mistake, according to the primary laws of their being, in every complication that occurs." And elsewhere, "Action, without will or effort, is to us, constituted as we are, unrealizable, unknowable, inconceivable." The monads of Leibnitz and the demons of Maxwell express in words the personality implied in every manifestation of force.

In this imperfect sketch of the increased resources and the present attitude of the physical sciences I have not aimed to speak as an advocate; much less to sit as a judge. The great problem of the day is, how to subject all physical phenomena to dynamical laws. With all the experimental devices, and all the mathematical appliances of this generation, the human mind has been baffled in its attempts to construct a universal science of physics. But nothing will discourage it. When foiled in one direction, it will attack in another. Science is not destructive, but progressive. While its theories change, the facts remain. Its generalizations are widening and deepening from age to age. We may extend to all the theories of physical science the remark of Grote which Challis quotes in favor of his own: — "its fruitfulness is its correctibility." Instead of being disheartened by difficulties, the true man of science will congratulate himself in the words of Vauvenargues, that he lives in a world fertile in obstacles. Immortality would be no boon if there were not something left to discover as well as to love. Fortunate, thought Fontenelle, was

Newton, beyond all other men, in having a whole fresh universe before him, waiting for an explanation. But science wants no Alexanders weeping because there are not other worlds to conquer. For every heroic Columbus, who launches forth, in however frail a bark, upon untried oceans, seeing before him rich continents where others behold only a wilderness of waters, there are precious discoveries in reserve. Surely the time has not yet come when the men in any section in this Association can fold their arms and say: — It is finished. Unless our physicists are contented to lag behind and gather up the crumbs which fall from the rich laboratories and studies of Europe, they must unite to delicate manipulation the power of mathematical analysis. Mathematics wins victories where experiment has been beaten. With good reason we applaud the many brilliant successes of instrumental research. Mathematical analysis, with its multitudinous adaptations, is the only key which will fit the most intricate wards in the treasury of science. With the help of her mathematical physicists, Great Britain has now taken a position in science which she has not held before since the days of Newton. In Germany, the physicists do not hold back from the most difficult problems of the day, because they are led along by experiment on one arm and by mathematics on the other. The zeal of the Italian scientists prevails, over even the terrors of Vesuvius, and makes them ready to become martyrs, like Pliny the elder, to nature and humanity. France, too, out of the very ashes of her humiliation, sends an inspiring word to us. Since her defeat, her scientific spirit has been aroused as it was after the days of the first revolution. Her Association for the Advancement of Science is only a two year old infant; but it has sprung into existence, like Minerva from the head of Jupiter, fullgrown and equipped. Already it has displayed a vitality and a prosperity which this Association, in its opening manhood, has not yet acquired. The words of its first President are as true for the United States as for France: — that the strength and glory of a country are not in its arms but in its science.

THE METAMORPHOSIS OF FLIES.* II.

BY DR. AUGUSTUS WEISSMANN.

We now attempt to consider morphologically the phenomena of the development of the Muscidæ, as discussed in the foregoing chapters. In the first place, the theories above laid down seem to be thoroughly established, *i. e.* that we have in the metamorphosis of the flies nothing to do with a simple change of skin. Indeed, the additional metamorphosis is accompanied by a casting aside and new formation of the chitinous framework which underlies the body, but this has a very subordinate signification. The systems of organs of the larva disappear simultaneously, it may be completely, it may be only histologically; and out of the fragments the tissues build up a new body. It is a matter of doubt whether to consider the larva and pupa as one and the same individual, or whether we have not in reality an alternation of generations. I think that those who speak of a metagenesis in certain Echinoderms should regard this also as such, if with V. Carus,† we consider metamorphosis as a series of developments in which the animal, during a certain stage in its development, is provided with provisional organs; but metagenesis as that in which this whole developmental stage itself (Amme) must be considered as a provisional one, so that it readily follows that the system of organs of the larva taken collectively is provisional, or in other words, that the larva itself is a provisional stage, while the fly must be considered as a new individual; such process taking rank as an alternation of generations. At all events from the Pluteus-form larva new organs arise in the sea star, as from the larva in the fly. There is the alimentary canal and the water vascular system, which last, though in a plainly undeveloped condition, is already partially developed in the larva. Here we have, except the hypoderm of the abdominal segments of the larva, no parts which pass into the pupa without previously undergoing a total revolution. The alimentary canal and the water-vascular system of the Echino-

*Forming the closing chapter in "Die Entwicklung der Dipteren," 1864.

†System der thierischen Morphologie, Leipzig, 1853, p. 264.

derm larva at no time perform their function; they continue to develop more perfectly; they grow more complicated in their structure; they throw off their parts singly; they only hold in reserve their histological structure in order to become physiologically capable of performing their functions.

In the Muscidæ, on the contrary, each organ of the larva does not become entirely lost, though bordering upon a histolysis, *i. e.* becoming functionally incapable, their histological elements dissolve themselves into a blastema, from which a new histological element must arise. The only difference from the total destruction, such as befalls the muscles, the fat bodies, etc., is this, that the destruction of tissues here becomes a continuous process, and the new organs are built up out of the same material which composed the old ones. This obtains in the intestine, the nervous system and the dorsal vessel. But a surprising analogy to the development of the Echinoderm occurs in the formation of the imaginal disks. As the body of the Echinoderm selects at several points around the alimentary canal of the larva indifferent cell masses, and then all unite and consolidate into a single mass, so arise at different places within the body of the fly larva — here still in genetic relation with the organs of the larva — masses of indifferent cells, which become differentiated in the course of the development of the different parts of the imago, and become transformed into a common whole. It cannot be considered as an essential deviation, that in the Pluteus larva these cell masses are formed during the life of the larva, while in the muscid larva they are formed before that, in the egg; and this deviation occurs to such a slight extent, as we have seen above, that a pair of the formative disks, those out of which the upper half of the prothorax is formed, here makes an exception, and is only developed shortly before the pupation. Had we considered the formation of the cell masses of the Echinoderm larva as buds, then for a still stronger reason is the formation of the imaginal disks of the Muscidæ a budding process. They are outgrowths of the tegumentary membrane of the nerves and tracheæ of tissues, which, if they are not histologically, are yet physiologically equivalents to the complete fibrous tissues of the vertebrates. Both tissues are essentially derived from an amorphous, fundamental substance, which seems to have the capabilities which the more recent studies in histology ascribe to the peculiar restorative quality of the con-

nective tissue of vertebrates, *i. e.* of building up cells, and of returning to that from which it originated.

I find it generally true that the nuclei lying near a recent disk become metamorphosed into disk-forming cells; that they surround themselves with cells, and multiply like the other disk-cells. Such a process may be seen in the exceptionally late appearance of the upper prothoracic disks. These disks are already formed in the egg out of the embryo cells,—out of the same material as the tegumentary membrane with which they develop. They could only in an ideal sense be regarded as outgrowths of this tegumentary membrane; so the upper prothoracic disks are, in reality, nothing but outgrowths. The nuclei of the peritoneal skin form cells, and increase in number, constituting the disks. We have here a bud which scarcely differs from the buds which are concerned in the formation of new stigmata in the first moulting of the larva, and we should almost err in considering the morphological value of this disk to regard it as a true imaginal disk; it should at least not be compared with those of the free Tipulidæ larvæ, in which they have a by far more complicated structure, while they are considerably larger, and are indicated contemporaneously with the formation of the other disks of the thorax.

As I cannot agree with that opinion which regards the well-known metamorphosis of Echinoderms as a metagenesis, so am I still far from proposing that there is such a metamorphosis in the Muscidæ. We must certainly, with V. Carus and J. Müller, consider it in this respect as irrelevant whether the nurse produces one or more germs (in a monogenous way); whether the animal growing from the egg develops into a sexual form, or whether it, not capable of that, at the end of its development produces germs (buds) which build up a sexual animal; or whether the series of developmental forms from the embryo up to the sexually ripe animal end in one or two individuals. The answer in both cases seems to me not doubtful. In the Echinoderms as in the Muscidæ we have to deal with a metamorphosis, not with an alternation of generations. Larva and sexually mature animal are one and the same individual. In the Echinoderms it seems to me this is evident in that the internal organs (intestine and water canal system) are present, and *without any interruption of their functions* pass over from the larval stage to the adult sea star; so that a single germ will not from the beginning pass by gradual differen-

tiation into the perfected animal, but rather distinct accumulations of cells take place and in the course of development give rise to the new "individual."

But in the flies there is another fact which compels us to look upon the larva and pupa as a single individual, however slight may be the community in organs and external form between the two developmental stages. It is this circumstance that the same mass of organized substance constitutes the body of the growing larva, as of the fly. *During the metamorphosis no growth takes place.* The larval skin is thrown off, the insect forms a case around itself beneath whose shelter it remains and builds up the final form of the perfect insect. It takes in or gives out products of combustion of the still unceasing process of respiration. We have in a manner a second egg-development, and as we consider egg and larva as a single individual, so must the pupa, though not containing the undeveloped larva as a yolk, be considered as one and the same individual. But it does not happen that there is no stage in the development of the pupa in which the larval organs are not present; though the organs of flight are indeed newly formed, the exclusion of the larval body does not take place suddenly, but very gradually it grows parallel with a whole series of formations of new structures. Larva and fly overlap each other. There can be no doubt that they are one and the same individual; that their development also is to be considered as a metamorphosis.

It is still the most complete metamorphosis conceivable which we find in the Muscidæ, far more complete — I do not profess to speak from more recent observations — than, for example, the metamorphosis of the Lepidoptera. The destruction of the larval organs is in the Lepidoptera much less complete, as the lepidopterous pupa has the power of moving its hind-body. The muscles of the larval segments concerned in the movements seem to remain, and the nervous power is not interrupted; there is a communication between the nervous centre and the organism; the consciousness of the animal remains, it reacts on stimulation. According to Herold the pulsation of the dorsal vessel goes on. The pupa in short ceases not for a moment to be a living being, while the life of the Muscid is as latent as that of the fertilized egg.

Through the transfer of many organs of the larva into the pupa there is less need of the rebuilding of parts. I find that even in

the Lepidoptera *the origin of the thorax is from the hypodermis of the larva;* that no thoracic disks are developed within the body, but that the appendages of the thoracic segments grow by a direct metamorphosis from the limbs. The wings alone have a special mode of development and in a very peculiar way.

I think that the kind and mode of formation of the thorax in the pupa of insects have the closest connection with, at first sight, a very subordinate circumstance, the presence or absence of true limbs in the larva. *I think, that especially where the three segments of the larva immediately following the head bear appendages, the corresponding appendages—the legs—of the adult insect, are formed by a simple metamorphosis; while on the other hand, when these limbs are wanting in the larva, there are found thoracic scales within the body of the larva; and not only the appendages but also the walls of the body are new structures.*

All my observations, new and old, agree with this view; among others the earlier observations on the larva of the gnat, already spoken of, which have only anal limbs, but no typical segmental appendages, and in which the structure of the thorax is the same as in the Muscidæ.

When it is said that the life of the insect is latent in the pupa, that the usual functions of animal life, such as motion and sensibility are wanting, I might speak of the Muscidæ alone, and say this of all insects with a similar mode of formation of the thorax. There comes into consideration here, not only the greater or less independence of development, the more or less marked absence of the larval organs, but also the period of the formation of the pupa. In the Tipulid larva, noticed above, the larval and pupal states are more intimately blended than in the Muscids. The thorax and head of the pupa are already fully formed, while the larva is actively swimming about. When the larva skin is cast off the muscles of the pupa are already at work; the functions of animal life suffer no interruption.

But in the Muscidæ the loss of the larval parts precedes the formation of the body of the pupa. Hence we perceive no motion, and in fact the animal life is latent. The circulation of the blood ceases, the peripheral nervous system is destroyed, and the central system loses any power of action; at the same time all the inner organs become incapable of farther functional conditions. The development of new systems of organs begins with

the rise of indifferent cells in the mass to be sloughed off, much as happens in the formation of the blastoderm in the egg. This renewal of the structure is seen in a measure in the internal organs. We can distinguish four modes of development, according to which the parts of the fly originate. Either certain parts of the larvæ become persistent, under modification, or the larval organs become a foundation for the parts of a fly, but are thrown off, cell by cell, ere they assume a definite character. The third and last kind is when an entirely new development of parts takes place, the beginning of which either dates at the embryonic or larval period or at the close of the pupa state.

Let us consider this single mode of development more closely as regards the direct transfer of the larval parts alone from the hypodermis of the eight hinder larval segments, which are afterwards developed into the abdomen of the fly. The second mode occurs in the alimentary canal, the Malpighian vessels, with the dorsal vessel and the central part of the nervous system. The same process is observed in all these organs, that of their removal, cell by cell, with a succeeding new development. I might term it a *histolysis*. The histological elements of the organ—simple as well as compound—suffer a fatty degeneration; there remains a residuary mass filled mostly with fat molecules. In the nervous centres and Malpighian vessels the nuclei of the cells become persistent, and perhaps give rise to the development of new histological elements; whether this persistent mass deports itself like the alimentary canal, or whether after its destruction it retains the nuclei, must remain undetermined, nevertheless this same mass, which had composed the former organs, serves to build up anew the new ones. The products resulting from this destruction of organs are not wholly dispersed, but remain together and so receive the form of organs in their totality, even if no single histological element remain.

We must here leave undetermined how the cells which are to form the new organs originate, though this cannot be doubtful as regards the last two modes of development of the parts of the imago. They undergo a completely new development, *i. e.* of all those parts which are not generally present in the larva, or are not in their complete state of functional activity. In this last category belongs the thorax *and head of the fly and their appendages*, also those parts of the imago which arise from the disks;

also the *genital glands*. With a single exception all these parts are already formed in the egg, their cells also arise directly from the cells of the embryo, and in the upper prothoracic disks, which are afterwards formed, there is still a continuity between the embryonal cells and those of the disks if still indirect, through the mediation of the nuclei of the peritoneal membrane surrounding the tracheæ.

It is well to remark — and it is based on a grand plan in the development of the imago — that only the walls of the parts referred to arise from the formative disks, the skin alone and not the muscles. These last owe their origin to a new process of cell formation which generally obtains in the last half of the period of pupation, and gives the material for the development of the inner organs still wanting, the tracheæ, nerves, the fat body of the fly, and the genital armature. I have sought to call attention to the fact that we are dealing with a "free" cell formation, *i. e.*, that the newly formed cells arise directly or indirectly from cells. In the early part of these researches it was proved that the first cells in the fertilized egg show a process of free cell formation, arising independently from some previous formative element. An objection to this could be raised, that we have to do with an endogenous cell growth, while the whole egg should be considered as a cell. It is clearly shown to be in agreement with the earlier observations of Stein and Lubbock, that the insect egg is not the equivalent of a single cell, but is composed of a number of cells. Should this objection be laid aside, then a similar objection in the cell growth of the inner parts of the pupa could not arise, and if the relation be proved by fresh observations, then there may be a free cell growth in the living organism.

ENGLISH SPARROWS.

BY THOMAS G. GENTRY.

In the September issue of the NATURALIST, Dr. Brewer reviews at considerable length a small article which appeared in a previous number, from the pen of Dr. Coues, concerning the European

house sparrow. He accuses the latter of entertaining feelings of prejudice against the sparrows, and of being "apparently only too glad to condemn them on the scantiest evidence."

After a careful perusal of what Dr. Coues has written, I must confess that I am utterly unable to see how such a construction as Dr. Brewer's could be forced upon it. In the article to which reference has been made, Dr. Coues says, "I have always been opposed to the introduction of the birds, mainly on this score, also for other reasons." What the other reasons are, it is not my province to divine; but it is sufficiently obvious from a clause of the above quotation, that his opposition to them was not founded upon imaginary wrongs which he supposed they would commit, but upon knowledge either gained by personal observation or that had been communicated to him by others. He was undoubtedly in possession of evidence similar to mine, at the date of the latter's reception.

By referring to page 146 of the "Key," I find that he manifested considerable concern about the results which would evidently follow the overflowing of municipal limits by the species, when the latter should come in contact with our native birds. Here it is difficult to resist the conclusion, that the knowledge which he possessed at the time of writing the "Key," afforded a sufficient warrant for what he said, and furnished the ground for his anxiety. Dr. Coues is too careful an observer and recorder of facts to pen an article without having taken the precaution to intrench himself securely against assault. From the evidence submitted it is clear that the charge of prejudice which has been preferred against him, is not sustained.

Leaving the learned doctor to defend himself, in his own able manner, which I am assured he will not hesitate to do, a regard for my own feeble reputation, compels me to pen a few lines denunciatory of the charge of misrepresentation which Dr. Brewer has imputed to me, as implied in his review. The Doctor says in language not to be miscomprehended, that he "entirely discredits" my statement, assigning as the reason, that he does not believe that "the habits of either the house sparrow, or the robin, blue bird, or the native sparrows, are different in Pennsylvania from what they are in Massachusetts." What I see with the natural eye is evidence of belief. I am not prone to a state of "double vision," but generally observe things as they really exist.

I have never visited Massachusetts for the purpose of studying the habits of its birds, but have derived considerable knowledge thereof, from the writings of its ablest sons, and can fearlessly aver, that either they have failed to represent the facts as they found them, or else perceptible differences exist. I do not wish to be understood as imputing the charge of misrepresentation to them, but only to show that my experience in eastern Pennsylvania has been of such a character as to prove beyond dispute the existence of differences of habit. A multitude of circumstances exist to vary the habits of a species. A single circumstance occasionally suffices: but, generally, a combination is necessary to determine variation. The introduction of a new species in a given locality, in its struggle for existence, will often have a tendency to place a new aspect upon affairs. Rapidity of multiplication, and a consequent increase of numbers, will often make up for lack of individual courage;—for in union there is strength. Birds that are proverbial for courage will often betray feelings of cowardice, and yield when beset by fearful odds.

Ever since the introduction of the sparrows into our own city, I have been a close observer of their ways, and have watched their rapid increase and steady diffusion, with feelings of fear, lest coming into contact with our smaller birds, they would compel the latter to seek quarters elsewhere. This suspicion has been latterly confirmed. On the outskirts of Germantown, and even in the groves which surround many of our palatial residences, where the house sparrow has intruded, the robin, sparrows and blue bird, our most welcome guests, but occasionally greet us with their presence and voices. To be sure there are places where these denizens of foreign birth have not disarranged the quiet and harmony that once pervaded our groves and fields uninterrupted as they were save by an occasional breach of trust.

The members of a family that quarrel among each other will often exhibit the same unenviable trait of character towards outsiders. In the breeding season, Dr. Brewer admits that the males are exceedingly pugnacious, but only when actuated by amatory influence. Granting this for the sake of argument, is it not a reasonable presumption that while these influences endure, the passions being wrought up to a high pitch of excitement, the casual appearance of a stranger on the scene would be misconstrued as an act of interference, and the force of infuriated indig-

nation be wreaked upon the innocent intruder. This would cause the former source of trouble to be forgotten for the time being; a feeling of love and good will to prevail, and the united strength of several to be centred upon an apparently common foe.

A supposed case of this kind would seem to admit of no other explanation. Here would be the starting-point for that enmity which I know to exist between this species and our smaller birds in certain localities. As years roll on it will gradually strengthen and increase. Just such facts as this supposed case calls for, in order to lead to a satisfactory conclusion, fell under my immediate observation during the early days of last spring.

Our smaller birds were just as abundant then, as in former years, and, as far as I was able to determine, the circumstances by which they were environed were materially the same. In places not as yet overrun by the house sparrows, apparently circumstanced similarly, our native species build as freely as ever. But there are spots not a few in number, where year after year I have wended in my ornithological pilgrimages, and returned with a rich harvest; but latterly, they have been deserted, and the familiar forms and voices I once loved to greet have gone, and strangers now occupy their loved retreats.

I have known instances where our smaller birds have commenced nest-building, and on the appearance of the sparrows have been compelled to beat a precipitate retreat.

What I have thus detailed at length, has been the experience of others. Mr. Abel Willis of Germantown informs me that the robins and sparrows were frequent visitors upon his father's premises in early spring, and were wont to build upon the bushes and trees that occupy the lawn, for several years in succession. This last year they came as usual, but the house sparrows had preoccupied all the available places. Displeased with such presumption, they were not disposed to yield quietly, but set about to expel the intruders; and in the encounter, being outnumbered, were obliged to emigrate to other quarters.

Repeatedly they renewed their endeavors, but were as often defeated.

In the cavity of an old apple tree in Mr. Willis's yard, a pair of the imported sparrows built a nest early in April last, since which time they have successfully reared three broods of birds, and were engaged in preparing for a fourth, when the stump

which they occupied was severed from the trunk by the writer's request, and deposited in his collections. The birds were successful in bringing to maturity a family of twelve children. Mr. Willis informs me that the robins and sparrows were frequent visitors to the tree during the early part of the season, and their movements seemed to indicate a desire to build; but the coming of the house sparrows was the cause of a dispute in which the robins and our native sparrows were compelled to yield, and look for suitable quarters elsewhere. While engaged in nidification and incubation, he further informed me, not a bird is permitted to approach within ten paces of the nest, for the male is ever on the alert for intruders, and wreaks instant vengeance upon their temerity. The tree which held the nest being in close proximity to his residence, sitting within his back door, he could command a full view of the surrounding prospect, and observe every manœuvre of the sparrows. He had witnessed frequent encounters between the *innocent* sparrows and the robins and our native sparrows, and always noticed that the former were the aggressors. At the time of writing the sparrows still continue to visit the old apple tree, the scene of their former joys and pleasures.

The editor of the "Weekly Guide" of Germantown, last spring published the observations of several of our most prominent citizens, bearing upon this subject, in which were stated facts similar to what have been detailed. In the cases to which he referred, the sparrows were always the aggressive party, and what individual courage and strength failed to accomplish, was brought about through the medium of numbers. Instances were cited where the robins and blue birds were beset by numbers of the sparrows, and were completely banished from sites rendered dear and sacred by past associations and recollections. What our citizens particularly bewail is the utter repugnance which the sparrows manifest towards our smaller species. In certain localities referred to in the above publication, the sparrows had taken complete possession, and the familiar forms and voices of the song and chipping sparrows, the robin and blue bird, are recollections of the past, save when an occasional chirp bespeaks the presence of one of these friends, come back to take, perhaps, a last farewell of scenes which memory holds dear, to be instantly chased into obscurity.

Mr. John Strouse of Chestnut Hill, a careful observer of the habits of birds, a taxidermist by occupation, informs me that in all his experience, which has been a very long one, he never met a species which, for size, displayed such pugnacity and persistent pertinacity during the breeding period, as the house sparrow. Instances of these unenviable traits have repeatedly come under his immediate observation. There are localities, he affirms, known to him, where the robin, sparrows and blue bird, were wont to breed every spring, except the last, in great numbers. This neglect of accustomed sites he attributes to the quarrelsome nature of the sparrows. Upon the property of Mr. John Butler of Germantown, the smaller native species were always to be discerned in large numbers during the season of nidification, but now the English house sparrows have taken their places; the former were absolutely driven away, as he had ocular demonstration of the fact.

It is true that the birds have been of immense service in ridding our squares of the caterpillars which were once so prevalent and so annoying to persons of delicate nerves and refined tastes. Had our city authorities years ago, by a wise regulation, provided for the removal of the squirrels, and encouraged many of our smaller insectivorous birds to build, by erecting suitable boxes for their accommodation, and imposing the severest penalty upon any who should molest them, there is no doubt that we should have been saved the expense of importation and the repugnance which possesses some of us at witnessing the banishment of many of our most common species. Our smaller birds, once placed in our squares and unmolested, would, in course of time, come to regard such localities as their permanent homes, and, year after year, would revisit them under the spur of past associations. Innumerable instances might be cited to prove that birds are frequently so strongly attached to particular localities, that they visit them every season unless driven away perforce. Such being the case, why could not our squares be rendered so attractive to our smaller insectivorous birds that they would come to regard them as their homes during their sojourn with us? They could and the presence of the sparrows be dispensed with.

[We printed Dr. Coues note with much reluctance, believing that the introduction of the English Sparrow was an unmixed good. We make room for Mr. Gentry's reply, and *per contra* refer our readers to Mr. Gould's note on the English Sparrow in the present number of this Journal.—EDS.]

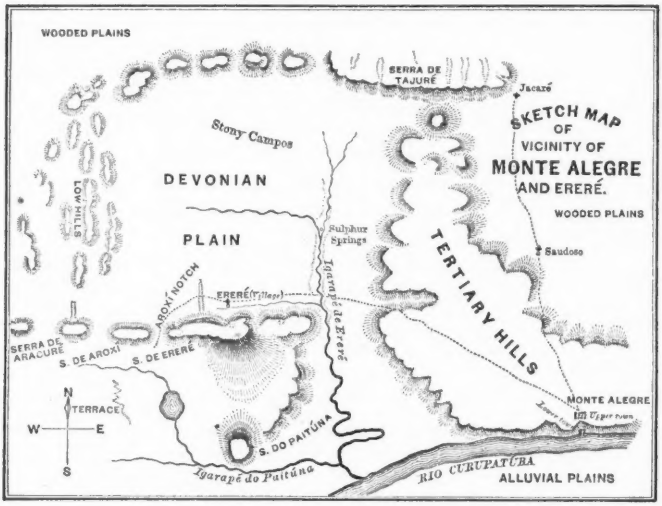
REVIEWS AND BOOK NOTICES.

THE GEOLOGY OF THE LOWER AMAZONAS.*—*The Ereré-Monte-Alegre District and the Table-topped Hills.* In this paper Prof. Hartt has given us a very interesting account of a part of his careful explorations on the Lower Amazonas. In the fall of 1870, a month was spent in the vicinity of Ereré with three assistants, and in the following year, nearly as much time was given to the same neighborhood in company with Mr. O. A. Derby, his present assistant. A large portion of the ground was gone over on foot and field notes and sketches carefully made. The results of Prof. Hartt's labors are of extreme interest, as they clear up many uncertainties in regard to the age of the sandstones and shales of Ereré, supposed by the late Prof. Agassiz to be of glacial origin. They really belong as far back as the Palæozoic age. Space will permit us to give but a brief outline of the main portion of the paper, in connection with the sketches illustrating it, the cuts of which have been kindly loaned by the Buffalo Society of Natural Science.

To the northwest of Monte-Alegre, a town situated on the Rio Curupatúba, near where it enters the Amazonas, and distant 350–360 miles nearly directly west of Pará, is quite an extensive plain, surrounded on all sides by hills and high grounds. The distance across the plain from north to south is about fifteen miles, its width from east to west is over ten miles. It lies somewhat higher than the alluvial plains of the Amazonas, and is drained by the Igarapé of Ereré, a small stream flowing into the Igarapé of Paitúna, which in turn enters the Rio Curupatúba some distance below Monte-Alegre. The structure of the plain of Ereré is very simple, for it is composed of nearly horizontal strata of Devonian age, through which the small Igarapé has worn a little valley, narrow toward the north where the stream flows over the bare rocks, but broader below and partly filled in with alluvial deposits, lying below the level of the plain. The rocks project in low bluffs along the edge of the valley, and about fifteen feet in thickness of the Devonian beds are exposed, where the road from the

*Contributions to the Geology and Physical Geography of the Lower Amazonas. By Ch. Fred. Hartt, Professor of Geology in Cornell University. Bulletin of the Buffalo Society of Natural Science, vol. I, No. iv, Jan., 1874, pp. 201–235.

Igarapé to Ereré crosses the bluff on the west. The beds composing the lower part of this bluff consist of a "soft, well-laminated, fine-grained shale, dark gray in color, alternating with white or red layers, and consisting of a fine, more or less sandy silt, with an abundance of little flakes of mica." One species of Discina and two of Lingula were obtained from the variegated shales. The former has been identified with a New York species, *Discina lodensis* of Hall from the Genesee shale, and is very abundant. Above the shales is a heavy bed of red and white clay rock, containing only obscure markings, the whole having a slight



inclination to the southeast. In the northwestern part of the campo, Prof. Hartt found, that "the rock varies from a very hard, dark-colored, silicious shale, to a well-bedded, dark gray, compact, cherty rock, breaking with a conchoidal fracture." These were the lowest beds of the series examined. Fragments only of fossils were obtained from the more shaly portions. Following the Monte-Alegre trail eastward from the igarapé, light-colored shales with thin bands of a reddish sandstone, full of fossils, are found just before reaching the Monte-Alegre highlands. The fossils belong to common Devonian genera. In a large open campo, to

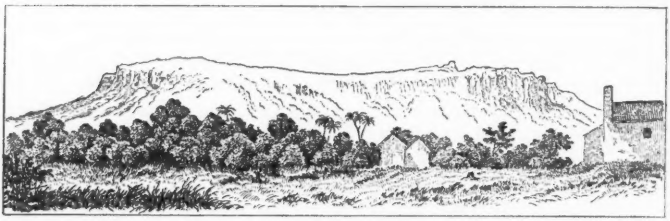
the north of the town of Ereré, at a distance of about two miles are red and whitish sandstones with whitish or yellowish shales. The sandstone is seldom seen *in situ*, but usually occurs in loose angular fragments. It affords an abundance of fossils, and forms the best collecting ground known on the plain. At this point Prof. Hartt and Mr. Derby made a very large collection of fossils. It represented ten genera of Brachiopods, about the same number of genera of Lamellibranchs and Gasteropods, two species and genera of Trilobites and several other forms. After a careful comparison of these with North American and European collections, several of the species were found to be identical with species occurring in the Hamilton group of New York, and described by Prof. Hall. So the conclusions are, that the sandstones and shales of the Ereré plain were formed at a period, corresponding to that in which the Hamilton shales were laid down, in North America.

The surface characters of the plain are in strict keeping with its simple structure. To the eastward of the Igarapé, it stretches to the foot of the Monte-Alegre highlands, almost as level as a floor, the Palæozoic rocks passing beneath the highlands. To the westward, as we approach Ereré, the plain forms a flat or rolling open campo, with long gentle ascents and descents. There is very little soil on any part of the plain, the surface of which is usually covered with angular fragments of red sandstone or rounded iron nodules. The campo is sparingly covered with grass, while the trees are few, stunted and scattered. Several large dykes occur on the plain.

Having gone over, though very hastily, what seems to be the most interesting part of Prof. Hartt's paper, as it is the richest in results, let us, by the aid of his sketches and descriptions, examine the surrounding hills. They form almost a square, protecting the level plain on all sides; to the eastward lie the Tertiary hills of Monte-Alegre, made up in large part of a single ridge, and extending from the Curupatúba on the south nearly to the Serra of Tauajurí on the north. They have a height of about 500 or 600 feet, are composed of horizontal beds of clays and sands, probably of Tertiary age, and seem to be "a degraded outlier of the once extensive formation of the serras of Parú." The upper town of the villa of Monte-Alegre is placed upon its summit, above the Rio Curupatúba, toward which the descent is very rapid.

The Serra of Ereré forms a part of the southern boundary of the plain. On its east, between the serra and the igarapé, is a plateau, made up, at least in part, of obliquely laminated beds of

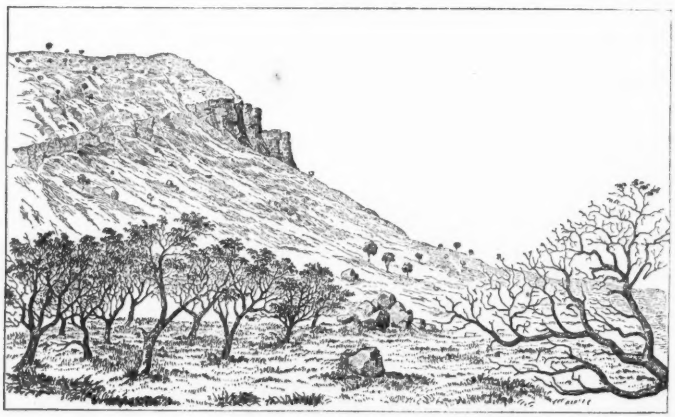
Fig. 93.



Serra of Ereré from the North.

tinted sands and clays. "The serra is a high, narrow, rugged, irregular ridge, four or five miles long, trending about east-north-east and west-south-west, and with abrupt and often precipitous sides." The top of the ridge is very irregular, composed of heavy beds of sandstone which are often exposed in ledges, or lie strewn

Fig. 94.



Serra of Ereré from the East.

about the surface in huge blocks. The sandstones form a line of bluffs along the upper part of the serra on its northern side, beneath which the slope is very rapid. At both ends the serra terminates

quite abruptly, as represented in the following cut showing it from the east.

Just west of the Serra of Ereré is a short ridge, with the same trend and geological structure, called Aroxí. This is followed by several still smaller ridges, apparently part of the same outcrop.

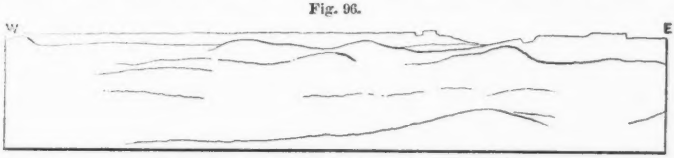
Fig. 95.

Serras of Ereré and Aroxí from the Southwest.

The Serra of Paitúna, near the igarapé of the same name, is composed of horizontal beds of the same sandstone as that of the Serra of Ereré, "so that the two serras probably form part of a synclinal fold." The sandstones forming the Serra of Ereré are well jointed and of unequal hardness. On weathering, they have assumed many curious forms, some of which are covered with Indian inscriptions and paintings. Fragments of silicified wood have been found in the stone, but they are too poorly preserved to admit of identification. Thus we have no palæontological evidence as to the age of these sandstones, which may be older or newer than those of the plain to the north.

Looking northeastward from the Serra of Ereré, beyond the northern termination of the Monte-Alegre highlands, we see the Serra of Tauajurí, a "splendid, blue, mountain mass, which, with precipitous front, heaves its back against the horizon, like a giant wave ready to break upon the level plains of Ereré, that lie spread

Fig. 96.



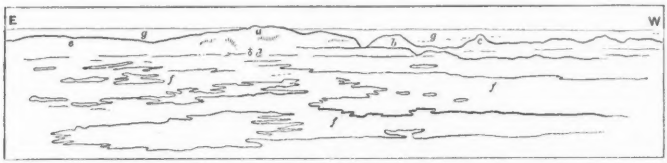
Sketch looking northward from Serra of Ereré.

out before us. While, northward from the hills (of Aroxí and Aracurí) stretches a belt of low, wooded ridges, skirting the campos on the west and north, and bending round to close the circuit with Tauajurí."

The Serra of Tauajurí is a sharp-crested ridge, exceedingly steep on the southern side, but sloping off at an angle of 10°–15° on the

northern. Its highest point is about 850 feet above the level of the sea, and it appears to differ entirely from Ereré in its geological structure. The view from the summit extends over an immense area, the entire Ereré-Monte-Alegre highlands and the Devonian plain being in sight.

Fig. 97.

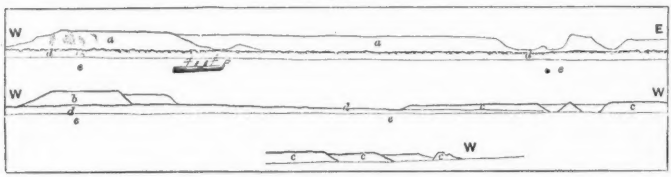


The Devonian Plain and Serras of Ereré from the Serra of Tauajurí.

The description of the table-topped hills to the eastward of Monte-Alegre, which have been the subject of much discussion, we copy verbatim:

"The table-topped hills of the Amazonas, so frequently described by travellers, consist of several isolated mountains or plateaus of circumdenudation composed of horizontal strata, which lie on the northern side of the river between Prainha and Almeirim, and known collectively as the Serras of Parú. They are characterized by their flat level tops and their very abrupt, sometimes precipitous sides. The westernmost of these serras is that of Parauáquára, eastward of which is that of Velha Pobre, while still farther east are the Serras of Almeirim. The general appearance of these mountains is represented in the following sketch made from the river."

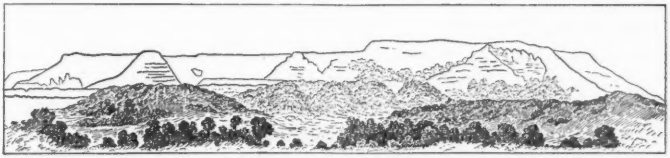
Fig. 98.



Serras of Parú from the Amazonas.

"The serra of Parauáquára is distant, as nearly as I can judge, about twenty miles to the eastward of the fazenda" (of L. J. Rodrigues on the Igarapé of Marapí). "It is an extensive, isolated plateau of circumdenudation, and apparently forms a long, narrow, irregular strip, running east–west. The following sketch, taken from a point a few miles west of the mountain, will show its topographical features as seen in elevation."

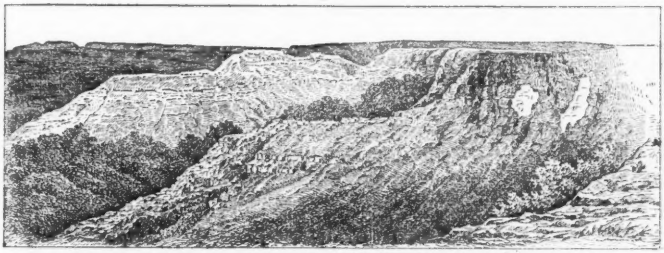
Fig. 99.



Serra of Parauáquára from the West.

"The following cut is from a sketch taken from the top of the serra, looking off northward along the western side, showing the level-topped summit, and the steep sides and spurs, along which run the edges of the horizontal strata like courses of masonry."

Fig. 100.



Serra of Parauáquára from the top looking northward.

No fossils have been found in the Parauáquára beds and their age is undetermined. — R. R.

We may add that the excellent palæontological work by Mr. Rathbun "On the Devonian Brachiopoda of Ereré, Province of Pará, Brazil," follows and supplements Prof. Hartt's paper, and is illustrated with three heliotypic plates, containing one hundred and one figures. The author draws the following conclusions from his study of the Brachiopod fauna of Ereré: —

"Although the fossils so far obtained from Ereré, were collected from so small an area and so limited a thickness of rock as to render it unsafe to draw any extended or definite conclusions from them; yet the Brachiopod fauna, such as it is, resembles so closely that of the Hamilton group of New York state, as to leave no doubt that the beds in which it was found, the sandstones and shales of Ereré, represent about the same horizon as the Hamilton group of North America. Not only are characteristic Hamilton group genera found in the Ereré beds, but even species of those same genera, which cannot be separated from North American species of the Hamilton group."

THE ORIGINAL DISTINCTION OF THE TESTICLE AND OVARY.* — An interesting contribution to the germ layer or Gastræa theory of Haeckel, which is now exciting the attention of embryologists and evolutionists, is afforded by Prof. E. Van Beneden of Liége. It will cause an inquiry into the real value of Haeckel's Gastræa theory, disputed by some of our leading zoologists. We translate Van Beneden's introduction and conclusions.

"Huxley was the first who demonstrated that the entire organization of the zoophytes, medusæ, and polypes, hydroids and Siphonophores can be reduced to a sac formed of two adjacent cellular layers, the ectoderm and entoderm (Allman), and who considered this proposition as expressing the general law of structure in the zoophytes.† Although one did not dream at this period of seeking homologies between the vertebrates and lower animals, Huxley took in all the bearings of his discovery. He recognized and formulated in clear and precise language his opinion on the homology which he believed exists between the ectoderm and entoderm of the Cœlenterata, and the two primordial cellular layers of vertebrates. See in what terms he expresses this idea; 'the peculiarity in the structure of the body-walls of the Hydrozoa, to which I have just referred, possesses a singular interest in its bearings upon the truth that there is a certain similarity between the adult states of the lower animals and the embryonic conditions of higher organizations.

'For it is well known that, in a very early state, the germ, even of the highest animals, is a more or less complete sac, whose thin wall is divisible into two membranes, an inner and an outer; the latter, turned toward the external world; the former, in relation with the nutritive liquid, the yolk. . . . . The various organs are produced by a process of budding from one, or other, or both of these primary layers of the germ.'

He seeks likewise to establish a parallelism, from a histological point of view between the ectoderm of zoophytes and the external layer of the embryo of vertebrates on one hand, and the endoderm and internal layer on the other. He concludes by saying: 'thus there is a very real and genuine analogy between the adult Hydrozoon and the embryonic vertebrate animal.' All the embryological researches made in late years, in the first phases of the embryonic development of animals of all branches, have tended to confirm, extending it to the whole animal kingdom. the opinion of the illustrious English naturalist. And in the first rank of

* De la Distinction originelle du Testicule et de l'Ovaire; Caractère sexuel des deux Feuillets primordiaux de l'Embryon; Hermaphrodisme morphologique de toute Individualité animale; Essai d'une Théorie de la Fécondation. Bruxelles, 1874. 8vo, pp. 68.

† Observations upon the Anatomy of the Diphydæ and the Unity of Organization of the Diphydæ and Siphonophoræ. Proceedings Royal Society, 1849.

work done in this direction may, without fear of contradiction, be cited that of Kowalevsky; in showing the identity of development of Amphioxus and of the Ascidians, he closed with a single stroke the abyss, thought to be impassable, which separates the branch of vertebrates from all the lower organisms. The important publications of the same author on the other types of organization, added to those of Gegenbaur, Haeckel, Ray Lankester, Kleinenberg and some others, have resulted in extending to the entire animal kingdom this grand conception that all the parts of the animal organism are formed from the two primordial cellular layers, and everywhere homologous.

These ideas have just been developed in detail and brilliantly defended in two essays of a high philosophic import. Haeckel has proposed in his brochure *Die Gastræa theorie, die phylogenetische Classification des Thierreiches und die Homologie der Keimblätter*, a theory which he had first announced in his monograph on the calcareous sponges. Some analogous ideas, and in several respects almost identical, have been published in England in the Annals and Magazine of Natural History, under the title; "On the Primitive Cell-layers of the Embryo as the Basis of the Genealogical Classification of Animals" by my friend E. Ray Lankester.

All the pluricellular animals, in which the development begins by the segmentation of the cell-egg, pass through in the course of their evolution a similar embryonic form, that of a sac whose thin walls are constituted of two adjacent layers; the endoderm and ectoderm. The first surrounds a cavity which is the primordial digestive tube; the second limits exteriorly the body of the embryo; it alone can be impressed by external causes. The digestive cavity communicates with the exterior by a single orifice which serves both as mouth and anus. The embryo is reduced to a digestive cavity, which is but a simple stomach; Haeckel has proposed to give to this primordial form the name of *Gastrula*. As this embryonic form occurs in the vertebrates, as well as the mollusks, arthropods, echinoderms, worms and polypes, it is clear that the ectoderm is homologous in the different types of organization; that the endoderm has in all the same morphological value; that the primordial digestive cavity of vertebrates, and that of all other types of organization have the same anatomical signification. The existence of this common form in the course of evolution of all the metazoal animals allows us to refer them to a common source; there is a convergence of the great types of organization and not a parallelism as had been urged by Cuvier and Von Baer. Finally, we can infer the existence at a geological epoch far back, of organisms like the Gastrula form; these organisms, probably varied in a thousand ways in their form and in their external characters, have been the common source of vertebrates, arthropods, mollusks, echinoderms, worms and zoophytes; they constitute the very numerous group of Gastræades (Haeckel).

If the endoderm and ectoderm are homologous in all the Metazoa [*i.e.* all animals except Protozoa] we then have a right to suppose that these two cellular layers have in all the same histological value, and that the same systems of organs are developed in the different types of organization from the same primitive layers. This induction has been already freely confirmed in that which concerns the central nervous system, which is developed in all animals from the ectoderm.

Consequently, it makes no difference if we should wish to know the origin of an organ, whether we seek for it in one or another type of organization; the results can be extended to the whole animal kingdom, and receive a general signification.

However, of all the types of organization, that which serves best for research on this capital question of the origin of organic systems, is that of the polypes, still called zoophytes or Cœlenterates. In them, in short, the ectoderm and endoderm persist with their embryonic characters during their entire life; all the organs of the zoophytes are only a dependence of one or the other of these layers, sometimes of the two layers united.

The polype form may be traced back with the greatest facility to the Gastrula, all the parts of which are preserved without undergoing any great modifications during all the course of existence.

Conclusions. In the Hydractiniæ 1. The eggs are developed exclusively from the epithelial cellules of the endoderm. They remain, up to the time of their maturity, surrounded by the elements of the endoderm.

2. The testicles and spermatozoa are developed from the ectoderm; this organ results from the progressive transformation of a primitive cellular fold formed by invagination.

3. There exists in the female sporosacs a rudiment of the testicular organ; in the male sporosacs a rudiment of an ovary. The sporosacs are then morphologically hermaphrodites. * * * * Fecundation consists in the union of an egg, a product of the endoderm, with a certain number of spermatozoa, products of the ectoderm. This act has no other end than to unite chemical elements of opposite polarity, which, after having been united an instant in the egg, separate again; for in most animals those in which the division of the vitellus into two occurs, the elements from which the ectoderm are formed are already separated from those which are to form the internal layer of the embryo.

The new individuality is realized at the instant when the union between the elements of opposed polarity has taken place, as absolutely as a molecule of water is formed by the union of atoms of hydrogen and oxygen."

In a late paper on the embryology of jelly fishes Metznikoff has criticised the universal application of Haeckel's Gastræa theory,

and shown that in the embryos of the Acalephs and Echinoderms the *outer* layer is invaginated and forms the walls of the stomach. And it appears to us that if organs, such as the ovary and testicle, almost universally recognized as homologous, are developed from opposite germ-layers, then the importance of the germ-layer theory of Haeckel is diminished by Van Beneden's remarkable discovery. We should say, however, that at the last meeting of the French Association for the Advancement of Science, Mr. P. Hallez questioned whether the ovary and testis were homologous. Meanwhile Giard announced at the same meeting that the male organs of *Sacculina*, a crustacean, are developed from the frontal glands, which in turn arise from the ectoderm.

Maps of Wheeler's Expedition. — We have received six advance sheets of a "Topographical Atlas" projected to illustrate Explorations and Surveys west of the 100th meridian of longitude, embracing results of the different expeditions under Lt. Wheeler, Corps of Engineers, U. S. A., published by the war department. The scale of each atlas sheet is one inch to eight miles. Sheet 50 covers portions of central and western Utah; sheets 58 and 59 embrace parts of eastern and southeastern Nevada and southwestern Utah; sheet 66 covers portions of southwestern Utah, northwestern Arizona and southeastern California. The work appears to be well done and will be useful to geographers and naturalists studying the distribution of plants and animals, especially the map of the areas of drainage to the Atlantic and Pacific oceans and of the interior basins of the United States, west of the Mississippi river.

We have found exceedingly useful the progress map of lines and areas lying west of the 100th meridian, and giving the lines of exploration by different parties sent out by government since Lt. Pike's expedition of 1805; not, however, including the area surveyed by parties under the Department of the Interior and Smithsonian Institution.

A large number of sheets are in preparation, and we shall ere long with the aid of these and the maps publishing by Hayden and Powell's expedition, with those of Whitney's Survey of California, be in possession of definite knowledge of the region west of the Mississippi, which is now altogether wanting in any atlas we have yet seen.

PHYSIOLOGY OF THE CIRCULATION.*—In this useful work the author has aimed "at producing a comprehensive view of the circulation as it exists in the lowest vegetable and highest animal forms." He has "endeavored to prove by a variety of arguments that the circulation, whenever and wherever found, differs less in kind than in degree; that fluids may move in living tissues with or without vessels and hearts; that the circulation in an aggregation of vegetable cells is essentially the same as that which occurs in the tissues of our own bodies. As a chain is composed of links, all of which are formed on a common type and fit into each other, so the circulation in the lowest vegetables and animals gradually develops into that of the higher, until we reach man himself; the circulation in the one being relatively as perfect as in the other."

BULLETIN OF THE CORNELL UNIVERSITY.†—The first two numbers of this new periodical, a credit alike to the university and the officers, contains a report of a reconnoissance of the Lower Tapajos river, by Professor C. F. Hartt, and a finely illustrated paper by Mr. O. A. Derby on the Carboniferous Brachiopoda of Itaitúba, Rio Tapajos, Brazil. We hope the patrons of the University will sustain this valuable publication.

MANUAL OF METALLURGY.‡—The author of this excellent manual was a student of Dr. Percy, the distinguished metallurgist, from whose work the present one is in part compiled. It will evidently prove, as the author hopes, a useful auxiliary to the more voluminous works on this subject. It is amply illustrated.

BOTANY.

INSECTIVOROUS PLANTS.§—The leaf of Sarracenia is a trumpet-shaped tube, with an arched lid, covering, more or less completely, the mouth. The inside is furnished with a perfect *chevaux-de-frise* of retrorse bristles, commencing suddenly about an inch from

* The Physiology of the Circulation in Plants, in the Lower Animals, and in Man. By J. Bell Pettigrew. Illustrated by 150 engravings on wood. London. Macmillan & Co. 1874. 8vo, pp. 329.

† Bulletin of the Cornell University. (Science.) Vol. I, Nos. 1, 2. Ithaca, N. Y., 1874. 8vo. pp. 63, with 9 plates.

‡ A Manual of Metallurgy. By W. H. Greenwood. Vol. i. Fuel, Iron, Steel, Tin, Antimony, Arsenic, Bismuth and Platinum. Illustrated by 59 engravings. New York. G. P. Putnam's Sons. Advanced Science Series. No date. [1874] 12mo, pp. 260. $1.50.

§ Abstract of a paper read at the Hartford meeting of the Amer. Assoc. Adv. Science.

the base; thence decreasing in size until from about the middle to the mouth they are so short, dense, and compact, that they form a decurved pubescence which is perfectly smooth and velvety to the touch, especially as the finger passes downward. Under the hood again, many of them become large and coarse. Running up the front of the trumpet is a broad wing with an emarginate border, parting at the top and extending around the rim of the pitcher. Along this border, but especially for a short distance inside the mouth, and less conspicuously inside the lid, there exude drops of a sweetened, viscid fluid, which, as the leaf matures, is replaced by a white, papery, tasteless, or but slightly sweetened sediment or efflorescence; while at the smooth bottom of the pitcher is secreted a limpid fluid possessing toxic or inebriating qualities.

The insects which meet their death in this fluid are numerous, and of all orders. Ants are the principal victims, and the acidulous properties which their decomposing bodies give to the liquid doubtless render it all the more potent as a solvent. Scarcely any other Hymenoptera are found in the rotting mass, and it is an interesting fact that Dr. Mellichamp never found the little nectar-loving bee or other Mellifera about the plants. On one occasion only have I found in the pitcher the recognizable remains of a *Bombus*, and on one occasion only has he found the honey bee captured. Species belonging to all the other orders are captured, and among the larger species I have found katydids, locusts, crickets, cockroaches, flies, moths, and even butterflies in a more or less irrecognizable condition.

Two species are proof against the siren influences of the destroyer, and in turn oblige it, either directly or indirectly, to support them. The first is *Xanthoptera semicrocea* Guen., a little glossy moth which may be popularly called the Sarracenia moth. It walks with perfect impunity over the inner surface of the pitcher, and is frequently found in pairs within the pitchers soon after these open in the early part of the season, or about the end of April. The female lays her eggs singly near the mouth of the pitcher, and the young larva from the moment of hatching spins for itself a carpet of silk, and very soon closes up the mouth by drawing the rim together with a delicate gossamer-like web, which effectually debars all small outside intruders. It then begins fretting under the hood, feeding downward on the cellular tissue and leaving only the epidermis, and by the time the worm has attained

its full size the pitcher generally collapses. At this time the worm is beautifully colored, and is characterized by rows of tubercles, which are especially prominent on the four larger, legless joints. The chrysalis is formed in a very slight cocoon. The species, kindly determined by Mr. A. R. Grote, was many years ago figured by Abbot, who found it feeding on *Sarracenia variolaris* in Georgia. Gueneé's descriptions were made from these figures, and here the author appends a few descriptive notes from the living material, of interest only to specialists on account of their technical character. The second insect which successfully braves captivation is a species of flesh-fly which the author names *Sarcophaga sarraceniæ*. After giving some technical details of structure, he shows how the larva of this fly riots in the putrid insect remains, and how, in order to undergo its transformations, it bores through the leaf and burrows into the ground. The immense prolificacy of these flesh-flies, and the fact that the young are hatched in the ovaries of the parent before they are deposited by her on tainted meat, are duly commented upon, as well as the rapid development of the species; also the propensity of the larvæ for killing one another and their ability to adapt themselves to different conditions of food-supply are made appreciable.

In conclusion the author says: To one accustomed to seek the why and wherefore of things the inquiry very naturally arises as to whether Xanthoptera and Sarcophaga play any necessary or important rôle in the economy of Sarracenia. Speaking of the Sarcophaga larva, Mr. Ravenel asks, "May he not do some service to Sarracenia as Pronuba does to Yucca?" And if so may not all this structure for the destruction of insects be primarily for his benefit? Can he be merely an intruder, sharing the store of provision which the plant, by ingenious contrivance, has secured for itself, or is he a welcome inmate and profitable tenant? Self-fertilization does not take place in Sarracenia, and the possibility that the bristly flesh-fly aids in the important act of pollination, lends interest to the facts. No one has witnessed with greater pleasure than myself the impulse which Darwin has of late years given to such inquiries, but the speculative spirit, is, in some quarters, becoming too wild and unbridled, and we should be cautious lest it impair our judgment or our ability to read the simple lesson of the facts. My own conclusions summed up are:

First: There is no reason to doubt, but every reason to believe

that Sarracenia is a truly insectivorous plant, and that by its secretions and structure it is eminently fitted to capture its prey.

Second: That those insects most easily digested (if I may use the term), and most useful to the plant, are principally ants and small flies, which are lured to their graves by the honeyed path, and that most of the larger insects, which are not attracted by sweets, get in by accident and fall victims to the peculiar mechanical structure of the pitcher.

Third: That the only benefit to the plant is from the liquid manure resulting from the putrescent captured insects, some of which doubtless descend to the root-stalk, and probably through large tubular cells, observed by Mr. Ravenel, passing through the petiole into the root.

Fourth: That Sarcophaga is a mere intruder, the larva sponging on and sharing the food obtained by the plant, and the fly attracted thither by the strong odor, as it is to all putrescent animal matter or to other plants, like *Stapelia variegata*, which give forth a similar odor. There is nothing to prove that it has anything to do with pollination, and the only insect that Dr. Mellichamp has observed about the flowers with any frequency, is a Cetoniid beetle — the *Euryomia melancholica*.

Fifth: That Xanthoptera has no other connection with the plant than that of a destroyer, though its greatest injury is done after the leaf has performed its most important functions. Almost every plant has its peculiar insect enemy, and Sarracenia, with all its dangers to insect life generally, is no exception to the rule.

Sixth: That neither the moth nor the fly have any structure peculiar to them, which enables them to brave the dangers of the plant, beyond what many other allied species possess. — C. V. Riley.

Distribution of American Woodlands.*— This is a paper to be published in the Statistical Atlas of the United States, now in progress of publication. It is an exhibition of a map, and a description of the methods by which the map was colored. Then follows an analysis of the trees of the tree flora in the ten districts into which the United States was divided. The flora of the United States, the author said, is believed to contain over 800

* Read at the Hartford Meeting of the Amer. Assoc. Adv. Science.

woody species, and over 300 trees. Of these trees about 250 species are somewhere tolerably abundant, about 120 species grow to a tolerably large size, 20 attain a height of 100 feet, 12 a height sometimes of over 200, and a few — perhaps 5 or 6 — a height of 300. The speaker analyzed the districts, adding: New England I consider to contain 80 or 85 species, of which 50 may reach a height of fifty feet; Maine is the great source of pine and spruce lumber, but as a whole the hard wood species predominate. Without going into the details of this flora, it is sufficient to give the author's remark that the area of woodland in New England is not perhaps greatly diminishing, but the amount of timber capable of being made into sawed lumber is lessening. The Middle States have about 100 to 105 species of trees, 65 to 67 of which sometimes reach 50 feet in height. Here were originally very heavy forests. There are still large areas heavily timbered, but the timber for all purposes is unquestionably rapidly diminishing, and there is no compensating influence going on for increase.

But in the Middle and New England States various hard woods attain their greatest perfection as to strength and durability; and as a consequence here we find the manufactures that are dependent on those woods. In the southeastern region — that is, extending from Virginia and Florida — we have about 130 species. In each case these form the conspicuous elements of the landscape. 75 attain a height of 50 feet or more, and about a dozen species a height of 100 feet. A belt of pine timber extends the whole length of this region, which is the great source of the hard and yellow pine. The author described the ornamental trees of that beautiful region, and resumed: The northwestern region, from Ohio to Minnesota, and north of the Ohio River, is represented by about 105 to 110 species, 68 or 70 of which may reach a height of fifty feet. That is the district furnishing at present the largest production of sawed lumber within the United States. Michigan alone furnished in 1870 of the 12,750,000 of M. feet, 2,250,000; Wisconsin furnished over 1,000,000 — the two states thus producing more than one-fourth of the whole yield returned in that year.

The author alluded to the rapidity of the destruction going on in that region; also of the diminution of sawing lumber in the forests, and the increase in woodland over the prairie region as it becomes cultivated. The southwestern region, extending from

Kentucky to Texas and the Gulf, has about 112 to 118 species, 60 or 65 of which attain a height of 50 feet, which the author also analyzed. West of these last two districts, this treeless belt, extending entirely across the continent from the Gulf of Mexico to the Arctic Ocean, is described, and its characteristics within the limits of the United States are mentioned. It is 350 miles wide in its narrowest part, between latitude 36° and 37°, and 800 miles wide on our northern border. The Rocky Mountain region is next considered. This consists of from 28 to 30 species, but a vastly smaller number making up the timber region. Perhaps not over half a dozen species constitute by far the larger part. No hard woods are abundant in any of the forests west of the Great Plains, although hard woods occur, particularly in the southern and western part, as scattered trees rather than as forests. Between the Rocky Mountains in the Sierra Nevada is a desert or sparsely wooded region, which extends southward to Mexico, uniting on its southern part with the treeless expanse which extends from the Atlantic to the Pacific along our southern frontier, and throwing out a spur entirely across the Rocky Mountains near the Pacific Railroad, connecting it with the treeless plains on the eastern side. This great treeless district varies very much in its different regions, has quite a large number of species of interest to botanists, and some few of economic value. The only forests within it are forests of coniferæ, occurring on the mountains, of which the largest one is in Arizona and is 400 miles long, the limits of which have recently been demonstrated by Lieut. Wheeler's Expeditions. Here followed an analysis by the author of the flora of the region west of this Desert; of California, Washington Territory, and Oregon, where were found the grandest forests, perhaps, on earth, and the noblest trees. The number of species of these latter is quite large, but in any one region the number of species is small. With one single exception all of the trees within the United States which attain a height of 200 feet are found in this district. The forests are entirely of cone bearing trees and the number of species is large, the number of timber trees being very large and their size and value also being great. In Washington Territory official reports state that the land will produce from 25,000 to 300,000 feet per acre, and that there are vast tracts "that would cover the entire surface with cord wood

10 feet in height." Then follows again an analysis of the trees of California and Oregon, including the many forms there that have been of interest in the world.

In Alaska, the tenth region or district, the data are insufficient for the map, but there are heavy forests there that are well known. The author rapidly discussed the original disposition of forests, showing what variety of causes have controlled this. Then the economic value of some of the industries directly dependent on them were alluded to, and the author ended his paper with some conclusions regarding the future supply, and suggestions regarding the planting of trees.—Wm. H. Brewer.

Adoxa Moschatellina L., in Iowa!—A correspondent in the Northeastern part of the state sent me some time since specimens which prove to be *Adoxa Moschatellina* L. Its locality is given in the "Flora of North America" as between lat. 54° and 64°, and on the higher peaks of the Rocky Mountains as far south as lat. 42°. Professors Porter and Coulter in the "Flora of Colorado" call it a "sub-alpine, common" plant. The last named gentleman collected it on Mt. Lincoln at the altitude of 13,000 feet. Mr. Watson in a private note says, "not found before this side of Colorado and the mountains, I believe."

Its occurrence in Iowa is certainly unlooked for. It grows abundantly on a rocky hillside, and was in bloom in May. Its locality in this case is best given as "Upper Iowa River, Iowa." —C. E. Bessey, *Agricultural College of Iowa*, Aug. 31, 1874.

Dispersion of Seeds by Shooting them off. — Our correspondent, Mr. Brandegee of Colorado, writes: —

"While drying seeds of *Ionidium lineare*, I noticed an interesting habit it has of shooting its seeds. Each capsule contains six seeds and is a six shooter. The three valves open wide and press the seeds tightly by their margins and in this way they are shot off singly, as one shoots orange seeds from between the fingers. A good shot will go fifteen to twenty feet."

All violets do it, and *Ionidium* is of this family. To render the operation clearer, it should be added that the three firm valves into which the capsule splits, after their separation fold together on their axis, to which the seeds are attached in a row, and it is the gradually increasing pressure so applied to the hard and smooth-coated ovoid seeds that fires them off. — Eds.

BOTRYCHIUM LUNARIA Swartz, was collected in Michigan long before the date given in the June NATURALIST by Mr. Gillman. In my herbarium are specimens collected on Isle Royale by Dr. A. E. Foote, in the summer of 1868.—C. E. BESSEY.

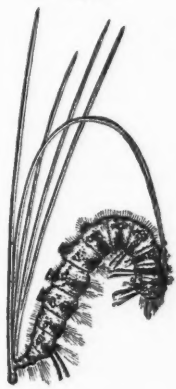
ZOOLOGY.

TRANSFORMATIONS OF OUR MOTHS.—Some interesting notes are given by Mr. J. A. Lintner in the "Twenty-sixth Annual Report on the New York State Cabinet of Natural History for 1872." He describes very fully the larva of *Eudryas unio* which feeds on *Epilobium coloratum*, and not on the grape, as stated by Fitch, and afterwards by Packard and Riley on Fitch's authority. Lintner gives characters for distinguishing the larvæ of *Eudryas unio* and *grata* as well as *Psychomorpha epimenis*, which so closely resembles Eudryas in its larval stage. The larvæ of *Parorgyia parallela* Gr. Rob., *Apatelodes angelica* Grote, *Cœlodasys unicornis* (Sm. Abb. Fig. 101), *Platycerura furcilla* Pack. (Fig. 102), *Dry-*

Fig. 101.

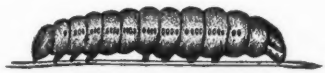
Larva of Cœlodasys unicornis.

Fig. 102.



Larva of Platycerura furcilla.

Fig. 103.



Larva of Nadata gibbosa.

Fig. 104.

Fig. 105.

Larva of Notodonta.

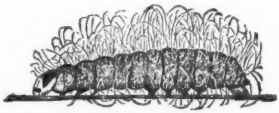
ocampa rubicunda Fabr., *Tolype Velleda* (Stoll), *Nadata gibbosa* (Sm. Abb. Fig. 103), and an unknown Notodonta (Fig. 104, Fig. 105, the same when feeding); also of *Cerura borealis* Boisd. (Fig. 106) and other Bombycid moths are described. Several

larvæ of the Noctuidæ are also described for the first time; among them *Diphtera deridens* Guenée (Fig. 107). Several new moths

Fig. 106.

Larva of Cerura.

Fig. 107.

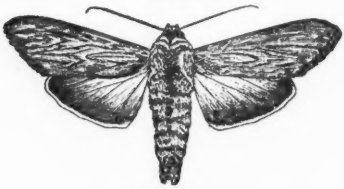


Larva of Diphtera deridens.

Fig. 108.

Male.

Fig. 109.



Female.

Cucullia Speyeri.

are described, among them *Cucullia Speyeri* (Fig. 108, male; Fig. 109, female). The separate copies are in some cases accompanied by the finest photographs we ever saw.

English Sparrow. — I noticed in your magazine for September Dr. Thos. M. Brewer's defence of the European house sparrow, and being convinced that this little friend of man has been maligned both in its native and adopted home, I desire to add my testimony as the result of careful experience and observation.

In January last I procured twelve European house sparrows in Boston, Mass., and had them sent to me by express. Two died from the effects of the journey, the rest I kept in my barn loft until April, when they were let out, or rather eight of them, as two more were killed by a cat which got in the barn without my knowledge.

On our place we have a large number of pear and peach trees, besides several maples, two walnut trees, an English oak and shrubs and flowering bushes of various kinds. In our kitchen garden we raised during the past season three kinds of corn, potatoes, cabbages, tomatoes, beets, carrots, onions, radishes, strawberry tomatoes, horse radish, celery, several varieties of beans,

peas, squashes, pumpkins, turnips, martynas, lettuce, spinach, and other vegetables, besides herbs, black, red and white currants, and several varieties of grapes; we also had a large number of flowers. During the summer our garden has been remarkably free from worms, and our crops never were better. Our trees never did better, while they have been remarkably free from caterpillars where last year (1873) they were nearly stripped of their foliage by their ravages.

In the place of the eight sparrows let out in April we now have thirty, and they appear to be constantly at work about the place. They are nearly always accompanied by the American goldfinch or yellow bird and our common sparrow.

To-day as I sat in my room writing I saw them fraternizing with a flock of blackbirds on one of our walnut trees. In fact they seem to court the society of other birds, and never have the birds been so abundant on our place. The male sparrows fight among themselves after the manner of roosters, but do not seem to molest other birds.

The sparrows did, with the yellow birds, attack our radish and turnip seeds as they ripened, but by using netting around those plants we kept the birds from doing serious damage. Nothing else was attacked by them, and we consider them a positive benefit to our place. We keep a horse and are accustomed to spread the stable droppings from day to day, broadcast. The sparrows seem to watch for this, and in an incredibly short time pick over and separate the manure and spread it much better than could be done with the hoe and rake. They are sprightly, friendly, and useful, and we would not have them leave us for much more than they originally cost. — Stephen Gould, *Newport, R. I., Sept.*, 1874.

Monstrosities among Beetles. — Dr. Kraatz publishes, in the 17th volume of the Berliner Entomologische Zeitschrift, an illustrated paper on deformities in beetles.

GEOLOGY.

Supposed Lower Silurian Land Plants. — Prof. J. S. Newberry doubts (American Journal Science and Arts, August, 1874) whether the Sigillaria mentioned by M. Lesquereux as occurring in the Lower Silurian beds of Ohio is a Sigillaria at all or whether it is a land plant even.

European Fossil Cetacea.—Prof. J. F. Brandt has published, in the memoirs of the Royal Academy of St. Petersburg, an elaborate quarto work on the fossil and sub-fossil Cetacea of Europe. It is illustrated with 34 plates.

ANTHROPOLOGY.

Restoration of Indian Pottery.—The caving of a bank of loamy earth on the east side of Connecticut River, about seven miles above Hartford, brought to light, several years ago, fragments of Indian pottery, which were found by a gentleman then temporarily residing at East Windsor Hill in that vicinity. They were composed of burnt clay intermingled with particles of pounded quartz, and as they evidently had a relation to one another, he commenced putting them together, using for that purpose slips of writing paper, about half an inch wide, and two inches long, coated with thick gum-arabic mucilage, and stuck on the inside of the pieces opposite the joints.

When I first saw his work he had reconstructed, from the pieces which he had found, about half of a kettle, the rim of which was entire, and about ten inches in diameter, and quite elaborately ornamented with lines grooved in the clay while it was in a plastic state. I was much interested in his work, for though I had frequently found fragments of that kind of pottery, I had never seen pieces of any one utensil sufficient in size or number to indicate the forms or dimensions of such ware. I went with him to the place of deposit, and we raked and sifted the soil thoroughly, and recovered additional fragments, from which we built up with the gummed slips, the entire form of the kettle, although there were in several places gaps which no shards were found to fill. It cost a deal of time and study to locate all the pieces, of which there were about seventy-five in number broken (it might almost be said) into every shape and size into which crockery could be broken. The labor was like that of putting together a dissected map, *very dissected;* or like that of solving all the figures of a book of Chinese puzzles laid together in one grand design.

The gummed slips answered the purpose of their designer admirably. Had he interposed between the shards any kind of cement, that would have prevented strict contiguity, or that would have set them immovably in their places as they were added one

by one, it would have been impossible to go on without trimming pieces that would not otherwise enter their proper places. Every additional piece trimmed would have aggravated the errors of adjustment, and the original form of the kettle would have been lost. But the paper slips, pasted and interlaced only on the inside of the work, allowed close adjustment of joints. They were also flexible, and capable of removal at any point by wetting if the progress of the work demanded. The fragments were pretty uniformly one-quarter of an inch in thickness, and it was sometimes, though not often, necessary to shore them up with props until the slips were sufficiently dry to hold them. The kettle was rebuilt resting on its rim, mouth downward, and in that position the fragility of the reconstruction made it necessary to keep it. The form developed was egg-shaped, the small end of the egg representing the bottom of the kettle, and the large end opening into a short, wide neck, which flared into a bell-shaped rim. The ornamental grooving was principally on the neck and rim; though the whole remaining exterior surface was chopped with short grooves intersecting at various angles. A cross section at any point did not vary very noticeably from a circle. The interior showed prints made by fingers in moulding and smoothing the plastic clay, and towards the bottom, the wear of subsequent use. The exterior of the bottom was somewhat darkened, as if by smoke; there was no glazing, and the general color was a dark brick red. The dimensions nearly were, height fourteen and one-fourth inches, diameter of rim nine and one-half inches, of neck eight and one-fourth inches, and of body eleven and one-half inches. Capacity about thirteen quarts, beer measure.

The first finder of the relic shortly transferred his residence to a foreign country, and his title to me. Wishing recently to take a drawing from this kettle, I found that atmospheric changes had caused most of the gummed slips to peel from the interior of the work so that it scarcely held together. Chaos might come again at any moment. As a first precaution I immediately made a quantity of duplicate numbers on little squares of paper, and gummed them on the outside opposite all the joints; then separated the work into nearly its original number of pieces, and rebuilt it with new slips, leaving a hole at the top (or bottom rather, as the kettle stood mouth downwards) large enough to see, and work upon, the interior.

After the body of the kettle had been brought into its true shape, the first step towards improvement was to make a not very thick solution of glue in water, and lay it with a small brush, into all the joints; avoiding spreading the glue beyond the joints as much as possible. This, on drying, set all the fragments in place quite firmly. The upper pieces around the hole, however, were not glued, but left supported by the gummed slips only for convenient insertion of the last pieces. Slips were next gummed on the inside across all gaps left by missing fragments. I then made a kind of cement, or mortar from pieces of very soft burned brick, pounded to dust, sifted, shaded to the color of the kettle with lamp-black, and moistened to a plastic state with not very thick glue water. With this mortar all the joints and gaps were filled on the outside. But the gaps were designedly not filled quite to the required thickness with one coat, as the mortar would shrink and crack somewhat in drying. Wherever these cracks appeared, glue was rubbed into them with the brush before laying on the final coat of cement. When this coat was dry it was smoothed with old files and sand paper and groove-marked in imitation of the unbroken surface. Protuberances in the cement too large to be readily filed down in a dry state, were first surface-softened by slight damping. The hole at the top was next underlaid with slips, and filled. Finally the slips were all removed from the interior by damping with a moist cloth, and any crevices that appeared were filled with cement.

From a basket full of nearly worthless shards was thus reconstructed a single relic, very rare (at least in Connecticut) whole and strong, showing no obvious breakage, as good as new for ethnological use, and as indestructible if not soaked with water, as any specimen of ceramic art.—E. W. Ellsworth, *East Windsor Hill, Ct.*

MICROSCOPY.

Beaded Silica Films.—Mr. Henry J. Slack has produced delicate films of silica, by mixing powdered glass, powdered fluor spar and sulphuric acid in a flask and conducting through a glass pipe the gas which escapes from the heated mixture into a dish containing glycerine and water. By contact with pure water the gaseous silica is deposited so suddenly and violently as to produce only amorphous particles, and a similar result is obtained

when silica is precipitated from its alkaline salts or water-glasses dissolved in water; but a mixture of glycerine retards the process and gives opportunity for the formation of definite forms. The films thus produced, washed and examined in water or mounted in balsam, either simulate organic cell forms, with cavities formed by the bursting of minute gas-bubbles, or consist of beads or spherules exhibiting remarkable regularity of size and arrangement; the beads seem to vary from $\frac{1}{30000}$ to $\frac{1}{100000}$ inch or less; and they appear to least advantage, in size, under the highest powers. They seem calculated to add to our knowledge of high power definition if not to throw light upon questions of crystallization and organization. Some of the films produced the beautiful polychromatic effects so often mentioned by Dr. Pigott as occurring in beaded diatoms and scales.

Cell-culture in the Study of Fungi. — Ph. Van. Tieghem and G. LeMonnier in their published researches on the Mucorini give a good working account of their method of cell-culture which is applicable not only to the smaller fungi but to many other plants. A glass cell $\frac{1}{5}$ or $\frac{1}{6}$ inch is cemented upon a glass slide, and a suitable cover-glass is kept in place by three minute drops of oil placed on the edge of the ring. The contained air is kept moist by a few drops of water placed in the bottom of the cell, while a very small drop of the nutritive fluid is placed on the lower surface of the cover-glass, and in this drop the spore to be cultivated is sown. The whole drop, and indeed the entire contents of the cell, can now be examined with suitable powers, and the germination and development of the plant traced hour after hour from any given spore, with the greatest certainty and ease. Extraneous spores will sometimes be introduced, but they are easily detected.

Handling Diatoms. — Capt. Lang, of the Reading Microscopical Society, gives "A useful Hint" to persons who select and arrange diatoms and pursue similar minute work under the microscope. Hairs, from various animals, whipped on to delicate handles, are generally satisfactory, those of the badger, or fine camel-hair or sable brushes, being generally useful. But some diatoms refuse to be thus picked up, and these he finds to be readily handled by means of a fine feather. The fine, stiff, elastic, and sharply pointed feathers on the extreme end of the carpal joints

of the wings of the golden plover and of the woodcock were found to be especially suitable.

Reproduction of Desmids.—Prof. Leidy, at a late meeting of the Academy of Natural Sciences of Philadelphia, made some remarks on the mode of reproduction and growth of the Desmids. In illustration he described a common species of Docidium or Pleurotænium. This consists of a long cylindroid cell constricted at the middle and slightly expanded each side of the constriction. When the plant is about to duplicate itself the cell-wall divides transversely at the constriction. From the open end of each half-cell there protrudes a colorless mass of protoplasm defined by the primordial utricle. The protrusions of the half-cells adhere together and continue to grow. The bands of endochrome now extend into the protrusions and subsequently keep pace with their growth. The protrusions continue to grow until they acquire the length and form of the half-cells from which they started. The exterior of the new half-cells thus produced hardens or becomes a cell-wall like that of the parent half-cells. In this condition two individuals of Docidium are frequently observed before separation. During the growth of the new half-cells the circulation of granules in the colorless protoplasm is quite active. In a species of Docidium $1\frac{1}{2}^{mm}$ long by $\frac{1}{10}^{mm}$ broad, the growth of the new half-cells was observed to be at the rate of about $\frac{1}{3}^{mm}$ in an hour.

Angular Apertures.—It is not yet forgotten that at the London examination of the $\frac{1}{6}$ inch lens sent to demonstrate the possibility of obtaining an excessive angular aperture in immersion work on balsam objects, the lens was measured at an adjustment of which nothing to the point was known except that it was not a position of immersion work at all, nor a recognized maximum position for any kind of work; the plain fact being that the accomplished committee were so bent upon teaching us the familiar fact of reduced angle that they seem to have forgotten to look for any other possibility in the case. Nor is it likely to be forgotten as long as Mr. Wenham so far forgets his usual and admirable caution as to allude to the correction of this palpable mistake as an "after quibble," nor while the eminent President of the Royal Microscopical Society utters in his formal address such an astounding statement as the following:—"The lens in this instance was properly corrected as a dry lens, and then after measurement in air it was

measured in water and then in very fluid Canada balsam without alteration of the adjustment. It may be quite possible that if the lens had been readjusted so as to give the best image for immersion in balsam, a slightly greater angle might have been obtained; but this would not have been a fair way of making a comparison as it is not the mode in which the glass would ever be employed in actual practice." By not saying squarely, *It is probably true that if the lens had been readjusted so as to give the best image for immersion in water, a greater angle would have been obtained; and this would have been the fair way of making the measurement, as it is the mode in which the glass would be employed in actual practice*, Mr. Brooke lost a rare opportunity to do a noble if not a generous act. As he is well known to be incapable of an intentional sophistry which by adroitly worded phrase should suggest a doubt where none is felt, belittle the concessions which are called for by manifest truth, and say one thing which is true but has no relation to the case at issue, and at the same time imply another thing which does relate to the case but is unqualifiedly incorrect, there is no choice but to conclude that his extraordinary statement, notwithstanding its tone of judicial coolness, was made without that deliberation which the official character of the address demanded.

On the other hand a still more recent lens by the same maker, claiming still more excessive aperture, has been examined by Mr. Wenham by his method of cutting off false light described in the August number of the NATURALIST. By this method, which would seem incapable of excluding any image-forming rays, he succeeded in obtaining a clear and distinctly limited angle for the lens whose light, when not thus protected, was vague and uncertain; the angular aperture at the same time being reduced from "180°" to "112°" which corresponded within a few degrees with the aperture computed trigonometrically from the width of the front lens and the length of the working focus. To this it is answered that with a dry object on the cover there is *no* distance involved and the triangle is impracticable; while accurate focussing upon a stop which is feasible at "uncovered" adjustment, is liable to error from spherical aberration when adjusted for maximum angle. Mr. Tolles' method of demonstrating the utilization of extra-limital rays is by placing a central stop upon the posterior surface of the back system of lenses, so large as to cut off all light when the objective is used dry; so that by no trick of illumination can the

light be made to pass through the narrow ring of clear aperture remaining around the stop; but if water be flowed in both above and below the balsam-mounted object, converting both the objective and the illuminating semi-cylinder into immersion arrangements, a well lighted and defined image is immediately produced. With regard to extreme angles in connection with dry objects, Mr. Tolles claims that his much-disputed $\frac{1}{6}$ inch does actually form an image with the most oblique rays that can impinge upon the slide, all other rays being cut off by a card or shutter which can be moved up close to the bottom of the slide.

A Finder for Microscopes with plain stage.—A writer in "Science Gossip" advises a horizontal line ruled across the centre of the stage from side to side. Vertical lines are ruled across this an inch each side of the centre. A large label, say nine-tenths of an inch square, is fixed to each end of the slide. When the object is in position these labels are marked with lines or dots to correspond with the stage-lines below, and by these can easily be returned to the same position on the same stage or any stage ruled exactly to match. Several objects may be indicated on the same slide by as many marks, and a memorandum preserved recording which marks indicate each object; thus 4-7 or $\frac{4}{7}$ records that the object is indicated by the fourth vertical and the seventh horizontal dot or line.

The Right-angled Prism as a substitute for the Mirror for transmitted light.— Mr. Ingpen uses an achromatic doublet, plano-convex, which can be placed when desired close to one side of the right-angled prism for transparent illumination, thus making the prism available as a substitute for both plane and concave mirrors, while the usual form where the condensing lenses are balsamed to the prism is useless for giving parallel light.

Apparatus for giving pressure to objects while drying.—Though spring clips of various kinds are chiefly employed for this purpose, yet a more compact and controllable arrangement is sometimes preferred. A mounting board is often arranged to hold the slides while pressure-rods tipped with cork rest upon the cover-glasses and give the required pressure; these rods being held in a vertical position by being passed through loops of wire or through a couple of perforated shelves one above the other, while they are pulled down with the required degree of force by elastic bands

passing over or through them and fastened to hooks or rings below. Such an apparatus, figured and described in "Martin's Manual of Microscopic Mounting," p. 28, is inconvenient chiefly by reason of the difficulty of varying pressure by means of the elastic band. Mr. C. E. Hanaman suggests the employment, as a substitute for the rods, of glass tubes loaded with shot or mercury so as to give the required pressure by their weight. By merely uncorking the tube and pouring a little mercury in or out all necessary changes of pressure may be secured, or the different tubes may be kept filled to different heights and the proper one chosen in each case.

THE NEW TYPE PLATE.—Möller has brought out another of his exquisite plates. This time he photographs, upon the centre of a glass cover, a square of about one-sixth of an inch composed of eighty circles surrounded by a black background with the name of a different diatom photographed under each circle. In the centre of each circle is mounted a diatom corresponding to the label below, two specimens being often introduced to show different views of the same form; of course all is arranged in inverted position on the slide, but under the microscope appears as described. The objects are mounted between two thin glasses which are set in a brass plate three and a quarter inches long and one and a quarter wide.

FIXING DIATOMS.—Mr. J. K. Jackson, in a communication to "Science Gossip" laments that the best "diatomaniacs" hold so tightly the secret of their mounting, and details his own experience for the assistance of others. The diatoms are carefully cleaned and a dip of the material containing them evaporated on a slip which is then placed under a 1½ inch objective. The covers on which to mount have been previously glazed with gum by putting on the centre of each, carefully cleaned, a small drop of a solution formed of an ounce of freshly distilled water and five or six drops of a freshly prepared solution of gum tragacanth or arabic; a number of covers being prepared at once on a wooden rack and dried over a hot plate in order to leave the least possible opportunity for exposure to the "vile inappreciable dust" of the room. With a hair from a cow's neck, mounted in a wooden handle, a diatom is picked out from the dip, at the rate of from eight to ten per minute and disengaged from the hair by dabbing it on

the glazed cover. When sufficient have been thus transferred to form a device, as a star, cross, initials, etc., they are placed under the 1½ inch and with the hair they are, with patience as well as tact, pushed, coaxed and driven into the required position, taking care to leave the valves on their backs to avoid insurmountable trouble with air bubbles. The objects are then fixed by bringing them close to the mouth and moistening by a long slow breath. After drying again on the hot plate they may be freely mounted in balsam which may even be boiled if desired and the mounting finished at once. Only on the calmest of days can sufficient immunity from dust be obtained for successful work; and the care of the eyes should never be forgotten during this straining work.

The Podura Scale.—Mr. Charles Brooke, in his President's Address before the Royal Microscopical Society, gives the following cool and excellent criticism on this much debated subject. "The writer, reviewing this subject under the dictates of common sense, when observing the familiar Podura notes of admiration well defined and free from colour, cannot resist the inference that in the objective all aberrations are nicely balanced, and the object truly represented in the visual image; on the contrary, when the same object is viewed as rows of ill-defined beads loaded with colours, it is difficult to avoid suspecting that the appearance is a spectral illusion, resulting from some unexplained diffraction or interference; and this suspicion can hardly be dispelled from his mind by anything short of rigid mathematical demonstration."

NOTES.

Mr. John E. Gavit, President of the American Note Company of New York, died at his residence, Stockbridge, Mass., on the 26th of August, in the fifty-eighth year of his age. It is rare that one finds in the busy walks of life a man who, while filling an office demanding constant attention, unlimited resource of invention, executive ability and diplomacy as well, should yet find time to familiarize himself with the various branches of science, not only understanding them thoroughly, but capable of appreciating and discussing their bearings with those specially engaged in the subject. Of a man of such varied attainments, one would naturally ask, why he had not published the results of his work;—had not made known his inventions. Various reasons may be given: first the

unceasing and continuous demands of his business rendered it impossible for him to carry on an uninterrupted line of investigation. Above all things he abhorred the hasty publishing of novelties. In this respect he was perhaps hypercritical.

Nothing could be too complete for him, and to this demand on his part for as near approach to absolute perfection as possible, the country is indebted for the exquisite work presented by the Bank Note Company of which he was chief executive officer.

His power to detect merit was noteworthy, and many a young man can look back to Mr. Gavit for the incentive first given him to work, and to thank him too for placing the standard to be attained high above that level aimed at by most workers. While his science came in as a relief from his business duties, one was surprised to hear him converse freely on such diverse subjects as architecture and deep sea explorations, fertilization of flowers, geology, and the whole range of biological science.

To hear him was not to listen to what any intelligent man might know regarding such topics, but it was to gather the latest views and to hear something new. In fact one could never talk with him without seeing old facts placed in a new light and new facts added.

Mr. Gavit was best known as a microscopist, his collection of stands, objectives, and other apparatus being one of the finest in the country.—E. S. Morse.

It will be remembered that Capt. Hall went as far north as 82° 16′. This has been exceeded by two Austrian explorers, Payer and Weyprecht, who penetrated into the frozen sea north of Siberia and discovered two hundred nautical miles north of Nova Zembla a mountainous country with glaciers and some vegetation and game, which they named Francis-Joseph Land. This was about one thousand miles in length so far as observed, and the northernmost point reached was Cape Vienna in latitude 83°.

A lobster farm, as we learn from "Nature," has been established near Boston. On the seaward side it is closed by banks, having hatches or sluices so as to admit of the flow and ebb of the tide. Last summer about 40,000 lobsters of all sizes were deposited in this ground. In the winter 15,000 fine lobsters were sold. We should be glad to hear of the further success of this important undertaking.

"THE Sandwich Naturalist Association" was recently organized at Sandwich, Illinois. The following officers were elected:—Nahum E. Ballou, M. D. *President*, Prof. A. E. Bourne, *Secretary*, and Frank M. Webster, *Treasurer*.

THERE is a new floorcloth, said to be made of ground cork and glue on a foundation of canvas, which would be an economical substitute for sheet cork for lining insect boxes.—EGBERT BAGG, Jr., *Utica, N. Y.*

THE famous geologist, M. Elie de Beaumont, recently died in Paris at the age of 76. In 1856 he was made Perpetual Secretary of the French Academy, succeeding Arago.

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